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Read April 7, 1808.

The comet which we have lately observed, was pointed out to me by Mr. Pigott, who discovered it at Bath the 28th of September, and the first time I had an opportunity of examining it was the 4th of October, when its brightness to the naked eye gave me great hopes to find it of a different construction from many I have seen before, in which no solid body could be discovered with any of my telescopes.

In the following observations, my attention has been directed to such phenomena only, as were likely to give us some information relating to the physical condition of the comet, it will therefore not be expected that I should give an account of its motion, which I was well assured would be most accurately ascertained at the Royal Observatory at Greenwich.

The different parts of a comet have been generally expressed by terms that may be liable to misapprehension, such as the head, the tail, the coma, and the nucleus; for in reading what some authors say of the head, when they speak of the size of the comet, it is evident that they take it for what is often called the nucleus. The truth is, that inferior telescopes, which cannot show the real nucleus, will give a certain magnitude of the comet, which may be called its head; it includes all the very bright surrounding light; nor is the name of the head badly applied, if we keep it to this meaning; and since, with proper restriction, the terms which have been used may be retained, I shall give a short account of my observations of the comet, as they relate to the above-mentioned particulars, namely, the nucleus, the head, the coma, and the tail, without regarding the order of the time when they were made; the date of each observation, however, will be added, that any person who may hereafter be in possession of more accurate elements of the comet's orbit, than those which I have at present, may repeat the calculations in order to obtain a more correct result.

Of the Nucleus.

From what has already been said, it will easily be understood that by the nucleus of the comet, I mean that part of the head which appears to be a condensed or solid body, and in which none of the very bright coma is included. It should be remarked, that from this definition it follows, that when the nucleus is very small, no telescope, but what has light and power in an eminent degree, will show it distinctly.

Observations.

Oct. 4, 1807. 10-feet reflector. The comet has a nucleus, the disk of which is plainly to be seen.

Oct. 6. I examined the disk of the comet with a proper set of diaphragms, such as described in a former paper,* in order to see whether any part of it were spurious; but when the exterior light was excluded, so far from appearing larger, as would have been the case with a spurious disk, it appeared rather diminished for want of light; nor was its diameter lessened when I used only the outside rays of the mirror. The visible disk of the comet therefore is a real one.

Oct. 4. I viewed the comet with different magnifying powers, but found that its light was not sufficiently intense to bear very high ones. As far as 200 and 300, my 10-feet reflector acted very well, but with 400 and 500 there was nothing gained, because the exertion of a power depending on the quantity of light was obstructed,† which I found was here of greater consequence than the increase of magnitude.

Illumination of the Nucleus.

Oct. 4, 6ʰ 15′. The nucleus is apparently round, and equally bright all over its disk. I attended particularly to its roundness.

Oct. 18. The nucleus is not only round, but also every where of equal brightness.

Oct. 19. I see the nucleus again, perfectly round, well defined, and equally luminous. Its brilliant colour in my 10-feet telescope is a little tinged with red; but less so than that of Arcturus to the naked eye.

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* See Phil. Trans. for 1805, page 53. Use of the Criterion.

† See Phil. Trans. for 1800, p. 78.

Magnitude of the Nucleus.

Oct. 26. In order to see the nucleus as small as it really is, we should look at it a long while, that the eye may gradually lose the impression of the bright coma which surrounds it. This impression will diminish gradually, and when the the eye has got the better of it, the nucleus will then be seen most distinctly, and of a determined magnitude.

Oct. 4. With a 7-feet reflector I estimated the diameter of the nucleus of the comet at first to be about five seconds, but soon after I called it four, and by looking at it longer, I supposed it could not exceed three seconds.

Oct. 6. 10-feet reflector, power 221. The apparent disk of the comet is much less than that of the GEORGIAN planet, which being an object I have seen so often with the same instrument and magnifying power, this estimation from memory cannot be very erroneous.

Oct. 5. Micrometers for measuring very small diameters, when high magnifying powers cannot be used, being very little to be depended upon, I erected a set of sealing wax globules upon a post at 2422 inches from the object mirror of my 10-feet reflector, and viewed them with an eye-glass, which gives the instrument a power of 221, this being the same which I had found last night to show the nucleus of the comet well. I kept them in their place all the day, and reviewed them from time to time, that their magnitudes might be more precisely remembered in the evening, when I intended to compare the appearance of the nucleus with them.

On examining the comet, I found the diameter of its nucleus to be certainly less than the largest of my globules, which being ,0466 inch, subtended an angle of 3″,97 at the distance of the telescope in the day time.

Comparing the nucleus also with the impressions, which the view of the second and third had left in my memory, and of which the real diameters were ,0325 and ,0290 inch; and magnitudes at the station of the mirror 2″,77 and 2″,47, I found that the comet was almost as large as the second, and a little larger than the third.

Oct. 18. The nucleus is less than the globule which subtends 2″,77.

Oct. 19. The air being uncommonly clear, I saw the comet at 40 minutes after five, and being now at a considerable altitude, I examined it with 289, and having but very lately reviewed my globules, I judged its diameter to be not only less than my second globule, but also less than the third; that is, less than 2″,47.

Oct. 6. The 20-feet reflector, notwithstanding its great light, does not show the nucleus of the comet larger than the 10-feet, with an equal magnifier, makes it.

Oct. 28. My large 10-feet telescope, with the mirror of 24 inches in diameter, does not increase the size of the nucleus.

Oct. 6. Being fully aware of the objections that may be made against the method of comparing the magnitude of the nucleus of the comet with objects that cannot be seen together, I had recourse to the satellites of Jupiter for a more decisive result, and with my 7-feet telescope, power 202, I viewed the disk of the third satellite and of the nucleus of the comet alternately. They were both already too low to be seen very distinctly; the diameter of the nucleus however appeared to be less than twice that of the satellite.

Oct. 18. With the 10-feet reflector, and the power 221, a similar estimation was made; but the light of the moon would not permit a fair comparison.

Oct. 19. I had prepared a new 10-feet mirror, the delicate polish of my former one having suffered a little from being exposed to damp air in noctural observations. This new one being uncommonly distinct, and the air also remarkably clear, I turned the telescope from the comet to Jupiter's third satellite, and saw its diameter very distinctly larger than the nucleus of the comet. I turned the telescope again to the comet, and as soon as I saw it distinctly round and well defined, I was assured that its diameter was less than that of the satellite.

6^h 20′. I repeated these alternate observations, and always found the same result. The night is beautifully clear, and the moon is not yet risen to interfere with the light of the comet.

Nov. 20. With a 7-feet reflector, and power only 75, I can also see the nucleus; it is extremely small, being little more than a mere point.

Of the Head of the Comet.

When the comet is viewed with an inferior telescope, or if the magnifying power, with a pretty good one, is either much too low, or much too high, the very bright rays immediately contiguous to the nucleus will seem to belong to it, and form what may be called the head.

Oct. 19. I examined the head of the comet with an indifferent telescope, in the manner I have described, and found it apparently of the size of the planet Jupiter, when it is viewed with the same telescope and magnifying power.

With a good telescope, I saw in the centre of the head a very small well defined round point.

Nov. 20. The head of the comet is now less brilliant than it has been.

Of the Coma of the Comet.

The coma is the nebulous appearance surrounding the head.

Oct. 19. By the field of view of my reflector, I estimate the coma of this comet to be about 6 minutes in diameter.

Dec. 6. The extent of the coma, with a mirror of 24 inches diameter, is now about 4′ 45″.

Of the Tail of the Comet.

Oct. 18. 7^h. With a night glass, which has a field of view of nearly 5°, I estimated the length of the tail to be 3°¾; but twilight is still very strong, which may prevent my seeing the whole of it.

Nov. 20. The tail of the comet is still of a considerable length, certainly not less than 2½ degrees.

Oct. 26. The tail of the comet is considerably longer on the south-preceding, than on the north-following side.

It is not bifid, as I have seen the comet of 1769 delineated, by a gentleman who carefully observed it.*

Oct. 28. 7-feet reflector. The south-preceding side of the tail in all its length, except towards the end, is very well defined; but the north-following side is every where hazy and irregular, especially towards the end; it is also shorter than the south-preceding one.

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* Dr. Lind of Windsor.

The shape of the unequal length of the sides of the tail, when attentively viewed, is visible in a night glass, and even to the naked eye.

Oct. 31. 10-feet reflector. The tail continues to be better defined on the south-preceding than on the north-following side.

Dec. 6. The length of the tail is now reduced to about 23′ of a degree.

Of the Density of the Coma and Tail of the Comet.

Many authors have said, that the tails of comets are of so rare a texture, as not to affect the light of the smallest stars that are seen through them. Unwilling to take any thing upon trust that may be brought to the test of observation, I took notice of many small stars that were occasionally covered by the coma and the tail, and the result is as follows.

Oct. 26, 6ʰ 15′. Large 10-feet reflector, 24 inches aperture. A small star within the coma is equally faint with two other stars that are on the north-following side of the comet, but without the coma.

7ʰ 30′. The coma being partly removed from the star, it is now brighter than it was before.

Oct. 31, 6ʰ 5′. 10-feet reflector. A star in the tail of the comet, which we will call *a*, is much less bright than two others, *b* and *c*, without the tail.

Two other stars, *d* and *e*, towards the south of *b* and *c*, are in the following skirts of the tail, and are extremely faint.

7ʰ 20′. The star *c* is now considerably bright, the tail having left it, while *d*, which is rather more involved than it was before, is hardly to be seen.

7ʰ 50′. The star *a*, towards which the comet moves, is involved in denser nebulosity than before, and is grown fainter. *d* is involved in brighter nebulosity than before, but being near the margin, it will soon emerge.

8ʰ 35′. Being still more involved, the star *a* is now hardly visible.

e is quite clear of the tail, and is a considerable star; *d* remains involved.

9ʰ 10′. The star *d* is also emerged, but the comet is now too low to estimate the brightness of stars properly.

Nov. 25, 7ʰ 35′. There is a star *a* within the light of the tail, near the head of the comet, equal to a star *b* situated without the tail, but near enough to be seen in the field of view with *a*. The path of the head of the comet leads towards *a*, and a more intense brightness will come upon it.

8ʰ 46′ The star *a* is now involved in the brightness near the head of the comet, and is no longer visible, except now and then very faintly, by occasional imperfect glimpses; but the star *b* retains its former light.

Nebulous appearance of the Comet.

Dec. 6. The head of the comet, viewed with a mirror of 24 inches diameter, resembles now one of those nebulæ which in my catalogues would have been described, " a very large, brilliant, round nebula, suddenly much brighter in the middle."

Dec. 16. 7-feet reflector. The night being fine, and the moon not risen, the comet resembles " a very bright, large, irregular, round nebula, very gradually much brighter in the middle, with a faint nebulosity on the south preceding side."

Jan. 1. 1808. 7-feet. "Very bright, very large, very gradually much brighter in the middle."

If I had not known this to be a comet, I should have added to my description of it as a nebula, that the center of it might consist of very small stars, but this being impossible, I directed my 10-feet telescope with a high power to the comet, in order to ascertain the cause of this appearance; in consequence of which I perceived several small stars shining through the nebulosity of the coma.

Jan. 14. 7-feet. "Bright, pretty large, irregular round, brighter in the middle."

Feb. 2. 10-feet, 24-inch aperture. "Very bright, large, irregular round, very gradually much brighter in the middle." There is a very faint diffused nebulosity on the north-preceding side; I take it to be the vanishing remains of the comet's tail.

Feb 19. Considerably bright; about $\frac{1}{7}$ of the field $= 8' 26''$ "in diameter, gradually brighter in the middle." The faint nebulosity in the place where the tail used to be, still projects a little farther from the center than in other directions.

Feb. 21. Less bright than on the 19th; nearly of the same size; gradually brighter in the middle. The nebulosity still a little projecting on the side where the tail used to be.

Result of the foregoing Observations.

From the observations which are now before us, we may draw some inferences, which will be of considerable importance with regard to the information they give us, not only of the size of the comet, but also of the nature of its illumination.

A visible, round and well defined disk, shining in every part of it with equal brightness, elucidates two material circumstances; for since the nucleus of this comet, like the body of a planet, appeared in the shape of a disk, which was experimentally found to be a real one, we have good reason to believe that it consists of some condensed or solid body, the magnitude of which may be ascertained by calculation. For instance, we have seen that its apparent diameter, the 19th of October, at 6ʰ 20′, was not quite so large as that of the 3d satellite of Jupiter. In order therefore to have some idea of the real magnitude of our comet, we may admit that its diameter at the time of observation was about 1″, which certainly cannot be far from truth. The diameter of the 3d satellite of Jupiter, however, is known to have a permanent disk, such as may at any convenient time be measured with all the accuracy that can be used; and when the result of such a measure has given us the diameter of this satellite, it may by calculation be brought to the distance from the earth at which, in my observation, it was compared with the diameter of the comet, and thus more accuracy, if it should be required, may be obtained. The following result of my calculation however appears to me quite sufficient for the purpose of a general information. From the perihelion distance 0,647491, and the rest of the given elements of the comet, we find that its distance from the ascending node on its orbit at the time of observation was 73° 45′ 44″; and having also the earth's distance from the same node, and the inclination of the comet's orbit, we compute by these data the angle at the sun. Then by calculating in the next place the radius vector of the comet, and having likewise the distance of the earth from the sun, we find by computation that the distance of the comet from the earth at the time of observation was 1,169192, the mean distance of the earth being 1. Now since the disk of the comet was observed to subtend an angle of 1″, which brought to the mean distance of the earth gives 1″,169; and since we also know that the earth's diameter, which, according to Mr. Dalby, is 7913,2 miles,* subtends at the same distance an angle of 17″,2 we deduce from these principles the real diameter of the comet, which is 538 miles.

Having thus investigated the magnitude of our comet, we may in the next place also apply calculation to its illumination. The observations relating to the light of the comet were made, from the 4th of October to the 19th. In all which time the comet uniformly preserved the appearance of a planetary disk fully enlightened by the sun: it was every where equally bright, round, and well defined on its borders. Now as that part of the disk which was then visible to us, could not possibly have a full illumination from the sun, I have calculated the phases of the comet for the 4th and for the 19th, the result of which is, that on the 4th the illumination was 119° 45′ 9″ as represented in figure 1, and that on the 19th it had gradually increased to 124° 22′ 40″, of which a representation is given in figure 2. Both phases appear to me sufficiently defalcated, to prove that the comet did not shine by light reflected from the sun only; for had this been the case, the deficiency I think would have been perceived, notwithstanding the smallness of the object. Those who are acquainted with my experiments on small silver globules,* will easily admit, that the same telescope, which could shew the spherical form of balls, which subtended only a few tenths of a second in diameter, would surely not have represented a cometary disk as circular, if it had been as deficient as are the figures which give the calculated appearances.

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* See Phil. Trans. for 1791, page 239. Mr. Dalby gives the two semi-axes of the earth, from a mean of which the above diameter 7913,1682 is obtained.

If these remarks are well founded, we are authorised to conclude, that the body of the comet on its surface is self-luminous, from whatever cause this quality may be derived. The vivacity of the light of the comet also, had a much greater resemblance to the radiance of the stars, than to the mild reflection of the sun's beams from the moon, which is an additional support of our former inference.

The changes in the brightness of the small stars, when they are successively immerged in the tail or coma of the comet, or cleared from them, prove evidently, that they are sufficiently dense to obstruct the free passage of star-light. Indeed if the tail or coma were composed of particles that reflect the light of the sun, to make them visible we ought rather to expect, that the number of solid reflecting particles, required for this purpose, would entirely prevent our seeing any stars through them. But the brightness of the head, coma, and tail alone, will sufficiently account for the observed changes, if we admit that they shine not by reflection, but by their own radiance; for a faint object projected on a bright ground, or seen through it, will certainly appear somewhat fainter, although its rays should meet with no obstruction in coming to the eye. Now, as in this case, we are sure of the bright interposition of the parts of the comet, but have no knowledge of floating particles, we ought certainly, not to ascribe an effect to an hypothetical cause, when the existence of one, quite sufficient to explain the phenomena, is evident.

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* See Phil. Trans. for 1805, page 38, the 5th experiment.

If we admit that the observed full illumination of the disk of the comet cannot be accounted for from reflection, we may draw the same conclusion, with respect to the brightness of the head, coma, and tail, from the following consideration. The observation of the 2d of February mentions that not only the head and coma were still very bright, but that also the faint remains of the tail were still visible; but the distance of the comet from the earth, at the time of observation, was nearly 240 millions of miles,[*] which proves, I think, that no light reflected from floating particles could possibly have reached the eye, without supposing the number, extent, and density of these particles, far greater than what can be admitted.

My last observation of the comet, on the 21st of February, gives additional support to what has been said; for at the time of this observation, the comet was almost 2,9 times the mean distance of the sun from the earth.[†] It was also nearly 2,7 from the the sun.[‡] What chance then could rays going to the comet from the sun, at such a distance, have to be seen after reflection, by an eye placed at more than 275 millions of miles[§] from the comet? And yet the instant the comet made its appearance in the telescope, it struck the eye as a very conspicuous object.

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[*] 239894939.

[†] The sun's mean distance being 1, that of the comet was 2,89797.

[‡] The comet's distance from the sun was 2,68,3196.

[§] 275077889.

The immense tails also of some comets that have been observed, and even that of the present one, whose tail, on the 18th of October, was expanded over a space of more than 9 million of miles,* may be accounted for more satisfactorily, by admitting them to consist of radiant matter, such as, for instance, the aurora borealis, than when we unnecessarily ascribe their light to a reflection of the sun's illumination thrown upon vapours supposed to arise from the body of the comet.

By the gradual increase of the distance of our comet, we have seen that it assumed the resemblance of a Nebula; and it is certain, that had I met with it in one of my sweeps of the zones of the heavens, as it appeared on either of the days between the 6th of December, and the 21st of February, it would have been put down in the list I have given of nebulæ. This remark cannot but raise a suspicion that some comets may have actually been seen under a nebulous form, and as such have been recorded in my catalogues; and were it not a task of many years labour, I should undertake a review of all my nebulæ, in order to see whether any of them were wanting, or had changed their place, which certainly would be an investigation that might lead to very interesting conclusions.

Account of a new irregularity lately perceived in the apparent Figure of the Planet Saturn.

The singular figure of Saturn, of which I have given an account in two papers, has continued, for several reasons, to claim my attention. When I saw the uncommon flattening of the polar regions of this planet, in the 40-feet telescope, I ascribed it to the attractive matter in the ring,* and of its tendency to produce such an effect we can have no doubt; but as another circumstance, which was also noticed, namely, an apparent small flattening of the equatorial parts, cannot be explained on the same principles, I wished to ascertain what physical cause might be assigned for this effect, and with a view to an investigation of this point, I have continued my observations. The position of the ring, at the last appearance of the planet, however, proved to be quite unfavourable for the intended purpose; for the very parts, which I was desirous of inspecting, were covered by the passage of the ring over the disk of the planet in front, or were projected on the ring, where it passed behind the body.

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* 9160542.

In my attempts to pursue this object, I perceived a new irregularity in the Saturnian figure, which, I am perfectly assured, had no existence the last time I examined the planet, and the following observations contain an account of it.

Observations.

June 16, 1807. The two polar regions of Saturn are at present of a very different apparent shape. The northern regions, as in former observations are flattened; but the southern are more curved or bulged outwards.

I asked my son JOHN HERSCHEL, who after me looked at Saturn while I was writing down the above observation, if he perceived that there was a difference in the curvature of the north and south pole, and if he did, to mark on a slate how it appeared to him. When I examined the slate, I found that he had exactly delineated the appearance I have described.

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* See Phil. Trans. for 1805, page 276.

In a letter to a very intelligent astronomical friend,* who has one of my 7-feet reflectors, I requested the favour of him to examine both the polar regions of Saturn, and to let me know whether he could perceive any difference in the appearance of their curvature; in answer to which I received, the 23d of June, a letter inclosing a drawing, in which also the southern regions were marked as more protuberant, with a greater falling off close to the irregularity. My friend, with his usual precaution, called this an illusion; and it will be seen by and by, that we shall have no occasion to ascribe this irregularity to a real want of due proportion, or settled figure of the polar regions of Saturn.

June 22, 9^h 24′. I see the same curved appearance at the south pole of Saturn, which was observed the 16th.

June 24. The air is very clear, and all the most critical phenomena are very distinctly to be seen; the shadow of the ring towards the south upon the planet; the shadow of the body towards the north-following side upon the ring; the belts upon the body; the division of the two rings; and with the same distinctness, I also see the protuberance of the south pole.

My seeing this appearance, at present, is a proof that it is not a physical irregularity or distortion of only some particular spot on the polar regions; for, in that case, it could not have been seen this evening, as from the rotation of the planet on its axis, which is 10^h 16′, the space of the polar circle which is now exposed to our view, must have been very different from what I saw the 16th and 22d.

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* Dr. Wilson of Hampstead, late Professor of Astronomy at Glasgow.

Many observations were made afterwards, which all confirm the reality of this appearance.

It is so natural for us to reflect upon the cause of a new phenomena, that I cannot forbear giving an opinion on this subject. To suppose a real change in the whole zone of the planet, cannot be probable; it seems therefore that this appearance must be, as my friend calls it, an illusion. But since the reality of this illusion, if I may use the expression, has been ascertained by observation, it is certain that there must be some extrinsic cause for its appearance; and also that the same cause must not act upon the northern hemisphere. Now the only difference in the circumstances under which the two polar regions of Saturn were seen in the foregoing observations is the situation of its ring, which passes before the planet at the south, but behind at the north. The rays of light therefore which come to the eye from the very small remaining southern zone of the saturnian globe, pass at no great distance by the edge of the ring, while those from the north traverse a space clear of every object that might disturb their course. If therefore we are in the right to ascribe the observed illusion to an approximate interposition of the ring, we have, in the case under consideration, only two known causes that can modify light so as to turn it out of its course, which are inflection and refraction. The insufficiency of the first to account for the lifting up of the protuberant small segment of the northern regions will not require a proof. The effects of refraction on the contrary are known to be very considerable. Let us therefore examine a few of the particulars of the case. The greatest elevation of the visible segment above the ring did not amount to more than one second and three or four

Fig. 1.

Oct.r 4th

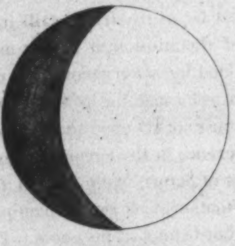
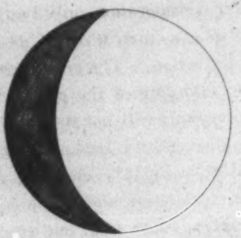


Fig. 2.

Oct.r 19th



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bands. Then supposing the ring, the edge of which is probably of an elliptical figure, to have a surrounding atmosphere, it will most likely partake of the same form; and the rays which pass over its edges will undergo a double refraction: the first at their entrance into the atmosphere, and the second at their leaving it; and these refractions may be such as to be sufficient to produce the observed elevation. For should they raise the protuberant appearance only half a second, or even less, the segment could no longer agree with the rest of the globe of Saturn, but must assume the appearance of a different curvature or bulge outwards.

The refractive power of the atmosphere of the ring has been mentioned in a former paper,* when the smallest satellites of Saturn were seen, as it were, attached to the narrow luminous line under which form the ring appeared when the earth was nearly in the plane of it; and the phenomenon, of which the particulars have now been described, appears to be a second instance in support of the same.

tenths. Then supposing the ring, the edge of which is probably of an elliptical figure, to have a surrounding atmosphere, it will most likely partake of the same form, and the rays which pass over its edge will undergo a double refraction: the first on their entrance into this atmosphere, and the second at their leaving it, and these refractions seem to be sufficient to produce the observed elevation. For should they raise the protuberant appearance only half a second, or even less, the segment could no longer range with the rest of the globe of Saturn, but must assume the appearance of a different curvature or bulge outwards.

The refractive power of an atmosphere of the ring has been mentioned in a former paper,* when the smallest satellites of Saturn were seen as it were bisected by the narrow luminous line under which form the ring appeared when the earth was nearly in the plane of it; and the phenomenon, of which the particulars have now been described, appears to be a second instance in support of the former.

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* See Phil. Trans. for 1790, page 7.

XIII. *Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood.* By Thomas Young, *M. D. For. Sec. R. S.*

Read May 5, 1808.

I. *Of the Friction and Discharge of Fluids running in Pipes, and of the Velocity of Rivers.*

Having lately fixed on the discussion of the nature of inflammation, for the subject of an academical exercise, I found it necessary to examine attentively the mechanical principles of the circulation of the blood, and to investigate minutely and comprehensively the motion of fluids in pipes, as affected by friction, the resistance occasioned by flexure, the laws of the propagation of an impulse through the fluid contained in an elastic tube, the magnitude of a pulsation in different parts of a conical vessel, and the effect of a contraction advancing progressively through the length of a given canal. The physiological application of the results of these inquiries I shall have the honour of laying before the Royal Society at a future time; but I have thought it advisable to communicate, in a separate paper, such conclusions, as may be interesting to some persons, who do not concern themselves with disquisitions of a physiological nature; and I imagine it may be as agreeable to the Society that they should be submitted at present to their consideration, as that they should be withheld until the time appointed for the delivery of the Croonian Lecture.

It has been observed by the late Professor Robison, that the comparison of the Chevalier Dubuat's calculations with his experiments is in all respects extremely satisfactory; that it exhibits a beautiful specimen of the means of expressing the general result of an extensive series of observations in an analytical formula, and that it does honour to the penetration, skill, and address of Mr. DUBUAT, and of Mr. DE ST. HONORÉ, who assisted him in the construction of his expressions. I am by no means disposed to dissent from this encomium; and I agree with Professor ROBISON, and with all other late authors on hydraulics, in applauding the unusually accurate coincidence between these theorems and the experiments from which they were deduced. But I have already taken the liberty of remarking, in my lecture on the history of hydraulics, that the form of these expressions is by no means so convenient for practice as it might have been rendered; and they are also liable to still greater objections in particular cases, since, when the pipe is either extremely narrow, or extremely long, they become completely erroneous: for notwithstanding Mr. DUBUAT seems to be of opinion, that a canal may have a finite inclination, and yet the water contained in it may remain perfectly at rest, and that no force can be sufficient to make water flow in any finite quantity through a tube less than one twenty-fifth of an inch in diameter, it can scarcely require an argument to show that he is mistaken in both these respects. It was therefore necessary for my purpose to substitute, for the formulæ of Mr. DUBUAT, others of a totally different nature; and I could follow DUBUAT in nothing but in his general mode of considering a part of the pressure, or of the height of a given reservoir, as employed in overcoming the friction of the pipe through which the water flows out of it; a principle, which, if not of his original invention, was certainly first reduced by him into a practical form. By comparing the experiments, which he has collected, with some of Gerstner, and some of my own, I have ultimately discovered a formula, which appears to agree fully as well as Dubuat's, with the experiments from which his rules were deduced, which accords better with Gerstner's experiments, which extends to all the extreme cases with equal accuracy, which seems to represent more simply the actual operation of the forces concerned, and which is direct in its application to practice, without the necessity of any successive approximations.

I began by examining the velocities of the water, discharged, through pipes of a given diameter, with different degrees of pressure; and I found that the friction could not be represented by any single power of the velocity, although it frequently approached to the proportion of that power, of which the exponent is 1.8; but that it appeared to consist of two parts, the one varying simply as the velocity, the other as its square. The proportion of these parts to each other must however be considered as different, in pipes of different diameters, the first part being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very minute tubes, while the second also becomes greater, for each given portion of the internal surface of the pipe, as the diameter is diminished.

If we express, in the first place, all the measures in French inches, calling the height employed in overcoming the friction f, the velocity in a second v, the diameter of the pipe d, and its length l, we may make $f = a\frac{l}{d}v^2 + 2c\frac{l}{d}v$; for it is obvious that the friction must be directly as the length of the pipe; and since the pressure is proportional to the area of the section, and the surface producing the friction to its circumference or diameter, the relative magnitude of the friction must also be inversely as the diameter, or nearly so, as DUBUAT has justly observed. We shall then find that a must be $.0000001 \left(430 + \frac{75}{d} - \frac{1440}{d+12} - \frac{180}{d+4}\right)$, and $c = .0000001 \left(\frac{900dd}{dd+1000} + \frac{1}{\surd d}\left(1050 + \frac{12}{d} + \frac{.9}{dd}\right)\right)$. Hence it is easy to calculate the velocity for any given pipe or river, and with any given head of water. For the height required for producing the velocity, independently of friction, is, according to DUBUAT, $\frac{v^2}{478}$, or rather, as it appears from almost all the experiments which I have compared, $\frac{v^2}{550}$; and the whole height h is therefore equal to $f + \frac{v^2}{550}$, or $h = \left(\frac{al}{d} + \frac{1}{550}\right) v^2 + \frac{2cl}{d} v$; and making $b = \frac{1}{al : d + .00182}$, and $e = \frac{bcl}{d}$, $v^2 + 2ev = bh$, whence $v = \surd\,(bh + e^2) - e$. In order to adapt this formula to the case of rivers, we must make l infinite; then b becomes $\frac{d}{al}$, and $bh = \frac{d}{a} \cdot \frac{h}{l} = \frac{ds}{a}$, s being the sine of the inclination, and d four times the hydraulic mean depth; and since e is here $= \frac{c}{a}$, $v = \frac{\surd(ads + cc) - c}{a}$, and in most rivers, v becomes nearly $\surd(20000\,ds)$.

In order to show the agreement of these formulæ with the result of observation, I have extracted, as indiscriminately and impartially as possible, forty of the experiments made and collected by DUBUAT; I have added to these some of GERSTNER's, with a few of my own; and I have compared the results of these experiments with DUBUAT's calculations, and with my own formulæ, in separate columns. There are six of DUBUAT's experiments which he has rejected as irregular, apparently without any very sufficient reason, since he has accidently mentioned that some of them were made with great care: I have therefore calculated the velocities for these experiments in both ways, and compared the results in a separate table.

Tabular Comparison of Hydraulic Experiments.

Observer.	d.	$\frac{1}{s}$	Superf. Veloc.	v.	Dub.	Log. ratio.	Y.	Log. ratio.	a $.1^7 \times$	c $.1^7 \times$	√ seconds)
Dubuat.	262.5	35723	15.96	12.56?	10.53	.0776	11.10	.0537	424	952	11.1
	258.5	6413	31.77	26.63?	28.76	.0334	28.02	.0221	424	952	28.3
	92.4	21827	9.61	7.01?	8.38	.0775	8.14	.0649	415	914	9.3
	75.6	27648	7.27	5.07?	6.55	.1112	6.27	.0923	413	887	7.5
	17.6	9288		5.70	5.86	.0120	5.97	.0291	376	465	6.1
	16.4	432		32.52	31.61	.0124	30.67	.0255	374	451	27.6
	11.7	1412		14.17	13.59	.0182	14.05	.0037	360	416	12.2
	9.9	427		22.37	24.37	.0372	24.41	.0379	355	414	21.7
	5.8	212		27.51	27.19	.0051	27.34	.0027	332	466	23.5

Observers.	d.	l.	h.	v.	Dub.	Log. ratio.	Y.	Log. ratio.	a $.1^7 \times$	$.1^7 \times$
Couplet	18	43200	145.08	39.16	40.51	.0148	38.49	0075	376	469
	5	84240	25.00	5.32	5.29	.0024	5.40	.0065	326	492
			16.75	4.13	4.23	.0103	4.21	.0083		
			5.58	2.01	2.25	.0490	2.01	.0000		
Bossut	2.01	2160	24	24.73	24.06	.0115	24.76	.0006	287	747
			12	16.38	16.10	.0075	16.86	.0125		
		1080	24	35.77	35.10	.0082	35.05	.0089		
		360	24	58.90	58.80	.0007	56.85	.0154		
	1.33	2160	12	12.56	12.75	0065	13.28	.0242	270	919
		1080	24	28.08	28.21	.0020	28.84	.0116		
		360	24	48.53	49.52	.0088	48.66	0015		
	1.	600	12	22.28	21.98	0055	22.83	.0106	259	1063
			4	12.22	11.76	.0167	11.92	.0108		
Dubuat		737	23.7	28.67	29.41	.0111	30.11	.0213		
			12.2	19.99	19.95	.0009	20.67	.0145		
			4.2	10.56	10.66	.0041	10.90	.0137		
		117	36	84.95	85.52	.0029	83.12	.0069		
			18	58.31	58.47	.0014	58.41	.0012		
	.24167	36.25	53.25	85.77	85.20	0029	85.71	.0003	309	2268
			41.25	73.81	73.90	.0005	74.67	.0050		
			20.17	51.96	50.14	.0155	50.87	.0093		
			5.00	23.40	23.19	0039	23.09	.0058		
			.83	7.58	8.22	.0420	7.22	.0212		
	.1667	36.25	51.25	64.37	64.95	.0031	64.08	.0021	402	2827
			38.75	54.19	55.32	.0090	54.93	0055		
			15.29	33.38	33.17	.0028	32.67	.0094		
			2.04	10.62	10.49	.0053	9.24	.0604		
	.125	34.17	42.17	45.47	46.21	.0070	45.88	.0039	518	3405
			35.33	41.61	41.71	.0010	41.55	.0006		
			14.58	26.20	25.52	.0114	24.94	.0214		
			2.08	7.32	8.35	.0572	6.98	.0206		

(Mean .0178 (Mean .0169
= L. 1.042) = L. 1.040)

Observers.	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
Gerstner, at 55.5°F.	.2	63	10.7	24.2	23.9	.006	24.1	.002	349	2533
			7.7	21.0	19.9	.023	19.1	.042		
			4.7	15.8	14.9	.026	13.9	.056		
			1.7	7.5	8.2	.039	6.9	.036		
			.7	2.5	5.0	.301	3.4	.133		
	.133	33	10.7	27.1	23.4	.064	22.5	.081	488	3259
			7.7	23.2	19.4	.077	18.5	.098		
			4.7	15.4	14.6	.024	13.5	.058		
			1.7	5.6	8.1	.160	6.7	.078		
			.7	2.3	4.6	.301	3.4	.169		
	.0674	33	10.7	10.0	8.9	.051	10.1	.004	975	5700
			7.7	7.2	7.4	.012	8.2	.057		
			4.7	4.5	5.6	.095	5.6	.095		
			1.7	1.5	3.1	.316	2.5	.222		
			.7	.5	1.8	.444	1.1	.342		
					(Mean	.129=L.1.346		.098=L.1.254)		
Y. At 60°.	$\frac{1}{21}$	8.50	32.4	14.40	0	∞	13.36	.032	2956	13882
	$\frac{1}{112}$	3.42	30.0	.53			.52	.098	13404	452100
		1.17	5.8	.27			.30	.046		
							(Mean	.029=L.1.068)		
Dubuat	2	255.25	36.35	86.31	84.2	.011	79.7	.035	287	747
	1	24	36.25	122.59	117.8	.018	120.8	.007	259	1003
			27	106.45	101.1	.022	104.1	.010		
			18	84.85	82.2	.013	84.8	.000		
			9	59.25	57.5	.013	59.7	.004		
		4	27.08	118.67	111.5	.027	118.5	.000		
					(Mean	.017=L.1.041		.009=L.1.022)		

It appears from this comparison, that in the forty experiments extracted from the collection, which served as a basis for Dubuat's calculations, the mean error of his formula is $\frac{1}{24}$ of the whole velocity, and that of mine $\frac{1}{25}$ only; but if we omit the four experiments, in which the superficial velocity only of a river was observed, and in which I have calculated the mean velocity by Dubuat's rules, the mean error of the remaining 36 is $\frac{1}{35}$, according to my mode of calculation, and $\frac{1}{37}$ according to Mr. DUBUAT's; so that on the whole, the accuracy of the two formulæ may be considered as precisely equal with respect to these experiments. In the six experiments which DUBUAT has wholly rejected, the mean error of his formula is about $\frac{1}{24}$, and that of mine $\frac{1}{45}$. In fifteen of GERSTNER's experiments, the mean error of DUBUAT's rule is one third, that of mine one fourth; and in the three experiments which I made with very fine tubes, the error of my own rules is one fifteenth of the whole, while in such cases DUBUAT's formulæ completely fail. I have determined the mean error by adding together the logarithmic ratios of all the results, and dividing the sum by the number of experiments. It would be useless to seek for a much greater degree of accuracy, unless it were probable that the errors of the experiments themselves were less than those of the calculations; but if a sufficient number of extremely accurate and frequently repeated experiments could be obtained, it would be very possible to adapt my formula still more correctly to their results.

In order to facilitate the computation, I have made a table of the coefficients *a* and *c* for the different values of *d*, all the measures being still expressed in French inches.

Table of Coefficients for French Inches.

d	a $.1^7\times$	c $.1^7\times$	d	a $.1^7\times$	c $.1^7\times$	d	a $.1^7\times$	c $.1^7\times$	d	a $.1^7\times$	c $.1^7\times$
∞	430	900	40	400	719	4	319	540	.4	257	1717
500	427	943	30	393	618	3	305	617	$\frac{1}{3}$	268	1895
400	426	946	25	387	560	2.5	296	687	.3	279	2008
300	423	950	20	380	492	2	288	751	$\frac{1}{4}$	303	2225
200	421	951	15	370	427	1.5	275	866	.2	349	2532
100	416	923	10	354	414	1	259	1063	$\frac{1}{6}$	402	2827
90	415	911	9	350	421	.9	255	1125	.15	440	3026
80	413	896	8	345	433	.8	252	1193	$\frac{1}{7}$	458	3116
70	410	872	7	340	440	.7	249	1278	$\frac{1}{8}$	518	3405
60	408	840	6	335	462	.6	248	1384	$\frac{1}{9}$	589	3693
50	406	792	5	325	512	.5	249	1524	.1	646	3985

For example, in the last experiment, where d is 1, l 4, and h 27.1, we have $a = .0000259$, $b = \frac{1}{al:d + .00182} = 516$, $c = .0001063$, $e = bcl:d = .22$, and $v = \sqrt{(bh + e^2)} - e = 118.46$, which agrees with the experiment within $\frac{1}{500}$ of the whole. I had at first employed for a the formula $\frac{430}{1 + 12:d} + \frac{57}{d} + \frac{1}{6dd}$, but I found that the value, thus determined, became too great when d was about 20, and too small in some other cases. Coulomb's experiments on the friction of fluids, made by means of the torsion of wires, give about .00014 for the value of c, which agrees as nearly with this table, as any constant number could be expected to do. I have however reason to think, from some experiments communicated to me by Mr. Robertson Buchanan, that the value of a, for pipes about half an inch in diameter, is somewhat too small; my mode of calculation, as well as Dubuat's, giving too great a velocity in such cases.

If any person should be desirous of making use of Dubuat's formula, it would still be a great convenience to begin by determining v according to this method; then, taking $b = \frac{l}{b - v^2 : 478}$, or rather, as LANGSDORF makes it, $b = \frac{l}{b - v^2 : 482}$, to proceed in calculating v by the formula $v = 148.5 (\sqrt{d} - .2) . \left(\frac{1}{\sqrt{b} - \text{H. L.} \sqrt{(b + 1.6)}} - .001\right)$, since this determination of b will, in general, be far more accurate than the simple expression $b = \frac{l + 45d}{b}$, and the continued repetition of the calculation, with approximate values of v, may thus be avoided. Sometimes, indeed, the values of v found by this repetition, will constitute a diverging instead of a converging series, and in such cases, we can only employ a conjectural value of v, intermediate between the two preceding ones.

Having sufficiently examined the accuracy of my formula, I shall now reduce it into English inches, and shall add a second table of the coefficients, for assisting the calculation. In this case, a becomes $.0000001 \left(413 + \frac{75}{d} - \frac{1440}{d + 12.8} - \frac{180}{d + .355}\right)$, $c = .0000001 \left(\frac{900dd}{dd + 1136} + \frac{1}{\sqrt{d}} \left(1085 + \frac{1321}{d} + \frac{1.0563}{dd}\right)\right)$, and $b = \frac{1}{al : d + .00171}$, e being $\frac{bcl}{d}$, and $v = \sqrt{(bh + e^2)} - e$, or $= \sqrt{\left(\frac{ds}{a} + \frac{cc}{aa}\right)} - \frac{c}{a}$, as before; and in either case the superficial velocity of a river may be found, very nearly, by adding to the mean velocity v its square root, and the velocity at the bottom by subtracting it.

Table of Coefficients, for English Inches.

d.	a. $.1^7\times$	c. $.1^7\times$	d.	a. $.1^7\times$	c. $.1^7\times$	d.	a. $.1^7\times$	c. $.1^7\times$	d.	a. $.1^7\times$	c. $.1^7\times$
∞	413	900	40	383	698	4	306	556	.4	254	1779
500	410	944	30	377	597	3	292	635	$\frac{1}{3}$	268	1963
400	409	948	25	371	526	2.5	284	694	.3	280	2082
300	406	951	20	364	482	2	277	774	$\frac{1}{4}$	305	2307
200	404	951	15	354	430	1.5	266	894	.2	354	2631
100	399	918	10	339	413	1	251	1099	$\frac{1}{6}$	409	2943
90	398	903	9	336	421	.9	248	1161	.15	447	3150
80	396	885	8	331	433	.8	245	1234	$\frac{1}{7}$	466	3251
70	393	860	7	327	449	.7	243	1322	$\frac{1}{8}$	528	3558
60	391	825	6	322	471	.6	243	1433	$\frac{1}{9}$	599	3866
50	389	772	5	312	507	.5	245	1578	.1	657	4183

II. *Of the Resistance occasioned by Flexure in Pipes or Rivers.*

Mr. DUBUAT has made some experiments on the effect of the flexure of a pipe in retarding the motion of the water flowing through it; but they do not appear to be by any means sufficient to authorise the conclusions which he has drawn from them. He directs the squares of the sines of the angles of flexure to be collected into one sum, which, being multiplied by a certain constant coefficient, and by the square of the velocity, is to show the height required for overcoming the resistance. It is, however, easy to see that such a rule must be fundamentally erroneous, and its coincidence with some experiments merely accidental, since the results afforded by it must vary according to the method of stating the problem, which is entirely arbitrary. Thus it depended only on Mr. DUBUAT to consider a pipe bent to an angle of 144° as consisting of a single flexure, as composed of two flexures of 72° each, or of a much greater number of smaller flexures, although the result of the experiment would only agree with the arbitrary division into two parts, which he has adopted. This difficulty is attached to every mode of computing the effect either from the squares of the sines or from the sines themselves; and the only way of avoiding it is to attend merely to the angle of flexure as expressed in degrees. It is natural to suppose that the effect of the curvature must increase, as the curvature itself increases, and that the retardation must be inversely proportional to the radius of curvature, or very nearly so; and this supposition is sufficiently confirmed, by the experiments, which Mr. Dubuat has employed in support of a theory so different. It might be expected that an equal curvature would create a greater resistance in a larger pipe than in a smaller, since the inequality in the motions of the different parts of the fluid is greater; but this circumstance does not seem to have influenced the results of the experiments made with pipes of an inch and of two inches diameter: there must also be some deviation from the general law, in cases of very small pipes having a great curvature, but this deviation cannot be determined without further experiments. Of the 25 which Dubuat has made, he has rejected 10 as irregular, because they do not agree with his theory: indeed 4 of them, which were made with a much shorter pipe than the rest, differ so manifestly from them that they cannot be reconciled: but 5 others agree sufficiently, as well as all the rest, with the theory which I have here proposed, supposing the resistance to be as the angular flexure, and to increase besides almost in the same proportion as the radius of curvature diminishes, but more nearly as that power of the radius of which the index is $\frac{7}{8}$. Thus if p be the number of degrees subtended at the centre of flexure, and q the radius of curvature of the axis of the pipe in French inches, we shall have $r = \frac{pv^2}{200000q}$ nearly, or, more accurately, $r = \frac{.0000045pv^2q^{\frac{1}{2}}}{q}$. These calculations are compared with the whole of DUBUAT's experiments in the following table.

Table of Experiments on the Resistance occasioned by Flexure.

p	q	v^2	r	B.	Y. 1	Y. 2	p	q	v^2	r	B.	Y. 1	Y. 2
288	3.22	15030	4.75		6.71	6.98	288	3.22	3415	1.50	1.57	1.52	1.58
		11330	3.50		5.06	5.26	144			.75	.78	.76	.79
		7199	2.33		3.21	3.34	72			.37	.39	.38	.39
		3510	1.08		1.56	1.62	196.5	6.12		.75	.78	.55	.62
216		7216	2.49	2.49	2.42	2.52	112.5	.53		1.50		3.63	3.00
144			1.50	1.66	1.61	1.67	720	3.22	5125	5.90	5.90	5.72	5.95
72			.75	.83	.80	.83	288		3458	1.64	1.59	1.54	1.60
196.5	6.12		1.50	1.66	1.16	1.31			860	.41	.40	.38	.40
147.4			1.12	1.24	.87	.98	—		821	.39	.38	.37	.38
98.3			.75	.83	.58	.65	288	4.10	3448	1.33		1.21	1.30
49.1			.37	.41	.29	.33			7449	2.90		2.59	2.78
112.5	.53		6.00		7.68	6.36	294.8	9.9					
99			5.90		6.74	5.60	360	4.1		8.64		8.08	8.62
288	3.22	3415	1.50	1.57	1.52	1.58	112.5	1.1					

In the last three experiments, the diameter of the pipe was two inches. The radius of curvature is not ascertained within the tenth of an inch, as DUBUAT has not mentioned the thickness of the pipes. The mean error of his formula in fifteen experiments, and of mine in twenty, is $\frac{1}{26}$ of the whole.

III. *Of the Propagation of an Impulse through an elastic Tube.*

The same reasoning, that is employed for determining the velocity of an impulse, transmitted through an elastic solid or fluid body, is also applicable to the case of an incompressible fluid contained in an elastic pipe; the magnitude of the modulus being properly determined, according to the excess of pressure which any additional tension of the pipe is capable of producing; its height being such, as to produce a tension, which is to any small increase of tension produced by the approach of two sections of the fluid in the pipe, as their distance to its decrement: for in this case the forces concerned are precisely similar to those which are employed in the transmission of an impulse through a column of air enclosed in a tube, or through an elastic solid. If the nature of the pipe be such, that its elastic force varies as the excess of its circumference or diameter above the natural extent, which is nearly the usual constitution of clastic bodies, it may be shown that there is a certain finite height which will cause an infinite extension, and that the height of the modulus of elasticity, for each point, is equal to half its height above the base of this imaginary column; which may therefore be called with propriety the modular column of the pipe: consequently the velocity of an impulse will be at every point equal to half of that which is due to the height of the point above the base; and the velocity of an impulse ascending through the pipe being every where half as great as that of a body falling through the corresponding point in the modular column, the whole time of ascent will be precisely twice as great as that of the descent of the falling body; and in the same manner if the pipe be inclined, the motion of the impulse may be compared with that of a body descending or ascending freely along an inclined plane.

These propositions may be thus demonstrated: let a be the diameter of the pipe in its most natural state, and let this diameter be increased to b by the pressure of the column c, the tube being so constituted that the tension may vary as the force. Then the relative force of the column c is represented by bc, since its efficacy increases, according to the laws of hydrostatics, in the ratio of the diameter of the tube; and this force must be equal, in a state of equilibrium, to the tension arising from the change from a to b, that is, to $b - a$; consequently the height c varies as $\frac{b-a}{b}$; and if the tube be enlarged to any diameter x, the corresponding pressure required to distend it will be expressed by a height of the column equal to $\left(1 - \frac{a}{x}\right) . \frac{bc}{b-a}$, since $\frac{b-a}{b} : c :: \frac{x-a}{x} : \left(1 - \frac{a}{x}\right) \frac{bc}{b-a}$. Now if the diameter be enlarged in such a degree, that the length of a certain portion of its contents may be contracted in the ratio $1 : 1 - r$, r being very small, then the enlargement will be in the ratio $1 : 1 + \frac{r}{2}$, that is, x' will be $\frac{rx}{2}$; but the increment of the force, or of the height, is $\frac{ax'}{xx} . \frac{bc}{b-a}$, which will become $\frac{ar}{2x} . \frac{bc}{b-a}$. Now in a tube filled with an elastic fluid, the height being b, the force in similar circumstances would be rb, and if we make $h = \frac{a}{2x} . \frac{bc}{b-a}$, the velocity of the propagation of an impulse will be the same in both cases, and will be equal to the velocity of a body which has fallen through the height $\frac{1}{2}b$. Supposing x infinite, the height capable of producing the necessary pressure becomes $\frac{bc}{b-a}$, which may be called g, and for every other value of x this height is $\left(1-\frac{a}{x}\right)g$, or $g-\frac{ag}{x}$, or, since b becomes $\frac{ag}{2x}$, $g-2b$, so that b is always equal to half the difference between g and the actual height of the column above the given point, or to half the height of the point above the base of the column.

If two values of x, with their corresponding heights, are given, as b and x, corresponding to c and d, and it is required to find a; we have $\frac{b-a}{b} : c :: \frac{x-a}{x} : d$, $dbx-dax = cbx-cba$, and $a = \frac{dbx-cbx}{dx-cb}$, or $\frac{b}{a} = \frac{dx-cb}{dx-cx}$. Thus if the height equivalent to the tension vary in the ratio of any power m of the diameter, so that, n being a small quantity, $x = b(1+n)$ and $d = c(1+mn)$, $\frac{b}{a} = \frac{bc\left((1+n).(1+mn)-1\right)}{bc\left((1+n).(1+mn-(1+n)\right)} = \frac{mn+n}{mn}$, since the square of n is evanescent, and $\frac{b}{a} = \frac{m+1}{m}$. For example, if $m = 4$, $\frac{b}{a} = \frac{5}{4}$, and if $m = 2$, $b : a :: 3 : 2$.

IV. *Of the Magnitude of a diverging Pulsation at different Points.*

The demonstrations of EULER, LAGRANGE, and BERNOULLI, respecting the propagation of sound, have determined that the velocity of the actual motion of the individual particles of an elastic fluid, when an impulse is transmitted through a conical pipe, or diverges spherically from a centre, varies in the simple inverse ratio of the distance from the vertex or centre, or in the inverse subduplicate ratio of the number of particles affected, as might naturally be inferred from the general law of the preservation of the ascending force or impetus, in all cases of the communication of motion between elastic bodies, or the particles of fluids of any kind. There is also another way of considering the subject, by which a similar conclusion may be formed respecting waves diverging from or converging to a centre. Suppose a straight wave to be reflected backwards and forwards in succession, by two vertical surfaces, perpendicular to the direction of its motion; it is evident that in this and every other case of such reflections, the pressure against the opposite surfaces must be equal, otherwise the centre of inertia of the whole system of bodies concerned would be displaced by their mutual actions, which is contrary to the general laws of the properties of the centre of inertia. Now if, instead of one of the surfaces, we substitute two others, converging in a very acute angle, the wave will be elevated higher and higher as it approaches the angle: and if its height be supposed to be every where in the inverse subduplicate ratio of the distance of the converging surfaces, the magnitude of the pressure, reduced to the direction of the motion, will be precisely equal to that of the pressure on the single opposite surface, which will not happen if the elevation vary inversely in the simple ratio of the distance, or in that of any other power than its square root. This mode of considering the subject affords us therefore an additional reason for asserting, that in all transmissions of impulses through elastic bodies, or through gravitating fluids, the intensity of the impulse varies inversely in the subduplicate ratio of the extent of the parts affected at the same time; and the same reasoning may without doubt be applied to the case of an elastic tube.

There is however a very singular exception, in the case of waves crossing each other, to the general law of the preservation of ascending force, which appears to be almost sufficient to set aside the universal application of this law to the motions of fluids. It is confessedly demonstrable that each of two waves, crossing each other in any direction, will preserve its motion and its elevation with respect to the surface of the fluid affected by the other wave, in the same manner as if that surface were plane: and, when the waves cross each other nearly in the same direction, both the height and the actual velocity of the particles being doubled, it is obvious that the ascending force or impetus is also doubled, since the bulk of the matter concerned is only halved, while the square of the velocity is quadrupled; and supposing the double wave to be stopped by an obstacle, its magnitude, at the moment of the greatest elevation, will be twice as great as that of a single wave in similar circumstances, and the height, as well as the quantity of matter, will be doubled, so that either the actual or the potential height of the centre of gravity of the fluid seems to be essentially altered, whenever such an interference of waves takes place. This difficulty deserves the attentive consideration of those who shall attempt to investigate either the most refined parts of hydraulics, or the metaphysical principles of the laws of motion.

V. *Of the Effect of a Contraction, advancing through a Canal.*

IF we suppose the end of a rectangular horizontal canal, partly filled with water, to advance with a given velocity, less than that with which a wave naturally moves on the surface of the water, it may be shown that a certain portion of the water will be carried forwards, with a surface nearly horizontal, and that the extent of this portion will be determined, very nearly, by the difference of the spaces described, in any given time, by a wave, moving on the surface thus elevated, and by the moveable end of the canal. The form of the anterior termination of this elevated portion, or wave, may vary, according to the degrees by which the motion may be supposed to have commenced; but whatever this form may be, it will cause an accelerative force, which is sufficient to impart successively to the portions of the fluid, along which it passes, a velocity equal to that of the moveable end, so that the elevated surface of the parts in motion may remain nearly horizontal: and this proposition will be the more accurately true, the smaller the velocity of the moveable end may be. For, calling this velocity v, the original depth a, the increased depth x, and the velocity of the anterior part of the wave y, we have, on the supposition that the extent of the wave is already become considerable, $x = \frac{ay}{y \mp v}$, taking the negative or positive sign according to the direction of the motion of the end; since the quantity of fluid, which before occupied a length expressed by y, now occupies the length $y \mp v$; and putting $a \sim x = z$, $z = \frac{av}{y \mp v}$. The direction of the surface of the margin of the wave is indifferent to the calculation, and it is most convenient to suppose its inclination equal to half a right angle, so that the accelerating force, acting on any thin transverse vertical lamina, may be equal to its weight: then the velocity y must be such, that while the inclined margin of the wave passes by each lamina, the lamina may acquire the velocity v by a force equal to its own weight; consequently the time of its passage must be equal to that in which a body acquires the velocity v, in falling through a height b, corresponding to that velocity: and this time is expressed by $\frac{2b}{v}$; but the space described by the margin of the wave is not exactly z, because the lamina in question has moved horizontally during its acceleration, through a space which must be equal to b; the distance actually described will therefore be $z \pm b$, and we have $\frac{z \pm b}{y} = \frac{2b}{v}$, $z \pm b = \frac{2by}{v}$, $av \pm by - bv = \frac{2byy}{v} \mp 2by$, $y^2 \mp \frac{3}{2} vy = \frac{av^2}{2b} - \frac{v^2}{2}$, $(y \mp \frac{3}{4} v)^2 = \frac{av^2}{2b} + \frac{v^2}{16}$; but, m being the proper coefficient, $v = m \sqrt{b}$, and $v^2 = m^2 b$, $\frac{av^2}{2b} + \frac{v^2}{16} = m^2 \left(\frac{a}{2} + \frac{b}{16}\right)$, $y = m \sqrt{\left(\frac{a}{2} + \frac{b}{16}\right)} \pm \frac{3}{4} v$, and $y \mp v = m \sqrt{\left(\frac{a}{2} + \frac{b}{16}\right)} \mp \frac{1}{4} v$. But when v is small, we may take $y \mp v$ nearly $m \sqrt{\frac{a}{2}}$, and $z = \frac{ma\sqrt{b}}{m\sqrt{(\frac{1}{2} a)}} = \sqrt{(2ab)}$, and $x = a \pm \sqrt{(2ab)}$, while the height of a fluid, in which the velocity would be y, is nearly $a \pm \frac{1}{2} \sqrt{(2ab)}$: consequently, when the velocity v is at all considerable, y must be somewhat greater than the velocity of a wave moving on the surface of the elevated fluid; and probably the surface of the elevated portion will not in this case be perfectly horizontal; but where v is small, y may be taken, without material error, $m \surd \frac{r}{2}$, or even $m \surd \frac{a}{2}$, which is the velocity of every small wave. The coefficient m is here assumed the same for the motion of a wave, as for the discharge through an aperture, and I have reason from observation to think this estimation sufficiently correct.

Supposing now the moveable end of the canal to remain open at the lower part as far as the height c, then the excess of pressure, occasioned by the elevation before it, and the depression behind, will cause the fluid, immediately below the moveable plane, to flow backwards, with the velocity determined by the height, which is the difference between the levels; and the quantity thus flowing back, together with that which is contained in the moveable elevation, must be equal to the whole quantity displaced. But the depression, behind the moveable body, must vary according to the circumstances of the canal, whether it be supposed to end abruptly at the part from which the motion begins, or to be continued backwards without limit: in the first case, the elevation z will be to the depression as v to $y - v$, the length of the same portion of the fluid being varied inversely in that ratio; in the second case, the proportion will be as $y + v$ to $y - v$: and the difference of the levels will be $z + z\frac{y-v}{v} = \frac{zy}{v}$, or secondly $z + z\frac{y-v}{y+v} = \frac{2zy}{y+v}$; and first, $m \surd \frac{zy}{v} c + (y - v) z = (a-c) v$; but, since y is here considered as equal to $m \surd \frac{a}{2}$, putting $\surd \frac{a}{2} - \surd b = d$, $y - v = md$, and, calling $a - c$, e, $m \surd \frac{zy}{v} c + mdz = me \surd b$, $\surd \frac{zy}{v} c + dz = e \surd b$, $c^2 \frac{zy}{v} = e^2b + d^2z^2 - 2dze \surd b$, $z^2 - \left(\frac{c^2y}{d^2v} + \frac{2e \surd b}{d}\right)z = -\frac{e^2b}{d^2}$, and, calling $\frac{c^2y}{2d^2v} +$

$\frac{e\sqrt{b}}{d}, f$, $z = f - \sqrt{\left(f^2 - \frac{e^2 b}{d^2}\right)}$: and in the same manner f is found, for the second case, equal to $\frac{e^2 y}{d^2 (y+v)} + \frac{e\sqrt{b}}{d}$. For example, suppose the height a 2 feet, $b = \frac{1}{4}$, $c = 1$, and consequently $e = 1$, then d becomes $\frac{1}{2}$, $v = 4$, and $y = 8$; and in the first case $z = .1$, and in the second $z = .14$.

If v, the velocity of the obstacle, were great in comparison with $m\sqrt{\frac{a}{2}}$, the velocity of a wave, and the space c below the obstacle were small, the anterior part of the elevation would advance with a velocity considerably greater than the natural velocity of the wave: but if the space below the obstacle bore a considerable proportion to the whole height, the elevation z would be very small, since a moderate pressure would cause the fluid to flow back, with a sufficient velocity, to exhaust the greatest part of the accumulation, which would otherwise take place. Hence the elevation must always be less than that which is determined by the equation $m\sqrt{zc} = ev$, and z is at most equal to $\left(\frac{ev}{mc}\right)^2 = \frac{e^2}{c^2} b$; but since the velocity of the anterior margin of the wave can never materially exceed $m\sqrt{\frac{x}{2}}$, especially when z is small, and $\sqrt{\frac{x}{2}}$ being in this case nearly $\sqrt{\frac{a}{2}} + \frac{e^2}{2\sqrt{(\frac{1}{2}a)c^2}} b$, $m\sqrt{\frac{x}{2}} - m\sqrt{b}$ $= m\left(\sqrt{\frac{a}{2}} + \frac{e^2 b}{\sqrt{(2a)}c^2} - \sqrt{b}\right)$ which, multiplied by z, shows the utmost quantity of the fluid that can be supposed to be carried before the obstacle. Supposing $b = \frac{1}{2}a$, this quantity becomes $m\sqrt{\frac{a}{2}} \cdot \frac{e^4}{c^4} \cdot \frac{a}{4}$; and if $\frac{e}{c}$ be, for example, $\frac{1}{10}$, it will be expressed by $\frac{1}{40000} av$, while the whole quantity of the fluid left behind.

A similar mode of reasoning may be applied to other cases of the propagation of impulses, in particular to that of a contraction moving along an elastic pipe. In this case, an increase of the diameter does not increase the velocity of the transmission of an impulse; and when the velocity of the contraction approaches to the natural velocity of an impulse, the quantity of fluid protruded must, if possible, be still smaller than in an open canal; that is, it must be absolutely inconsiderable, unless the contraction be very great in comparison with the diameter of the pipe, even if its extent be such as to occasion a friction which may materially impede the retrograde motion of the fluid. The application of this theory to the motion of the blood in the arteries is very obvious, and I shall enlarge more on the subject when I have the honour of laying before the Society the Croonian Lecture for the present year.

The resistance, opposed to the motion of a floating body, might in some cases be calculated in a similar manner: but the principal part of this resistance appears to be usually derived from a cause which is here neglected; that is, the force required to produce the ascending, descending, or lateral motions of the particles, which are turned aside to make way for the moving body; while in this calculation their direct and retrograde motions only are considered.

The same mode of considering the motion of a vertical lamina may also be employed for determining the velocity of a wave of finite magnitude. Let the depth of the fluid be a, and suppose the section of the wave to be an isosceles triangle, of which the height is b, and half the breadth c: then the force urging any thin vertical lamina in a horizontal direction will be to its weight as b to c; and the space d, through which it moves horizontally, while half the wave passes it, will be such that $(c-d).(a+\frac{1}{2}b) = ac$, when $c\ e\ d = \frac{bc}{2a+b}$. But the final velocity in this space is the same as is due to a height equal to the space, reduced in the ratio of the force to the weight, that is, to the height $\frac{bb}{2a+b}$, and half this velocity is $\frac{1}{2} m \surd \left(\frac{bb}{2a+b}\right)$, which is the mean velocity of the lamina. In the mean time the wave describes the space $c+d$, and its velocity is greater than that of the lamina in the ratio of $\frac{c}{d}+1$ to 1, that is $\frac{2a+b}{b}+1$ or $\frac{2a}{b}+2$ to 1, becoming $m\left(\frac{a}{b}+1\right)\frac{b}{\surd(2a+b)} = m\frac{a+b}{\surd(2a+b)}$; which, when b vanishes, becomes $m \surd \frac{a}{2}$, as in LAGRANGE's theorem, and, when b is small, $m\left(\surd\frac{a}{2}+\frac{3}{4}\frac{b}{\surd(2a+b)}\right)$, or $m\frac{a+\frac{1}{2}b}{\surd(2a)}$; but if a were small, it would approach to $m \surd b$, the velocity due to the whole height of the wave.

XIV. *A Letter on the Alterations that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of* Derry *and* Antrim. *Addressed to* Humphry Davy, *Esq. Sec. R. S. By* William Richardson, *D. D.*

Read March 17, 1808.

Sir,

I request you will be so good as to lay before the Royal Society, the following Observations on the Natural History of that part of Antrim, (contiguous to the Giant's Causeway,) which you and I examined so carefully together. I know not any country that deserves so well to have its facts faithfully recorded; from the important conclusions to which they lead.

The basaltic area (taken in its whole extent) comprehends the greater part of Antrim, and the east side of Derry to a considerable depth.

In a geological point of view, nature * has been very kind to this district, for not content with assembling together in a small space so many of her curious productions, and arranging them with more regularity and steadiness than in any other country described, she has condescended occasionally to withdraw the veil, and lay herself open to view, often exhibiting a spectacle equally gratifying to the admirer of magnificence, and to the curious naturalist, who can here, by simple inspection, trace the arrangements which are to be discovered elsewhere, only by penetrating beneath the surface of the earth.

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* By the word nature, which frequently occurs in the course of this Memoir, I always mean, according to Ray's definition, the wisdom of God in the creation of the world.

As soon as we enter the basaltic area, we begin to perceive traces of these arrangements; as we advance farther north, they increase; and in the tract near the shore, and especially at the island of *Rathlin*, which seems to have come fresher from the hand of nature than the rest of our area; the stratification of the whole is perfectly visible, and the nature of the several strata laid open to us at their abrupt and precipitous terminations.

To the southward we perceive the distinctive features abate, and wear away; the basaltic stratification indeed remains, but is no longer displayed to us in the same manner; the neat, prismatic, internal construction, of the strata, which occurs so frequently on, and near, the coast, is scarcely to be met with at a distance from it; a rude columnar appearance is all we find, and that but rarely.

It is at the periphery of our area, and especially at its northern side, that every thing is displayed to the greatest advantage; here we have perpendicular façades often continuous for miles, and every separate stratum completely open to examination.

Of these façades, four are more distinguished by their grandeur and beauty than the rest, *Magilligan Rock*, *Cave Hill*, *Bengore*, and *Fairhead.*

The two former are at the extreme points of the north-west diagonal of our area, and nearly forty miles asunder; they are at the summits of mountains, and accessible by land.

The precipitous faces of *Fairhead* and *Bengore*, to which had the pleasure of attending you, and which are visible only from the sea, are the most beautiful, and the most curious; for the strata, which at *Magilligan* and *Cave Hill*, are all nearly similar, at *Fairhead* and *Bengore* are much diversified. Of *Fairhead* I have already published an account in NICHOLSON'S JOURNAL, for December, 1801, and I now propose to execute an intention which I have had for some years of giving a minute account of *Bengore*.

I am aware that it will be extremely difficult to convey a clear and adequate idea of an assemblage of 16 strata, (for such is the number of which our promontory is composed), appearing and disappearing at various altitudes, yet retaining each its own proper place, and forming together a most beautiful and regular whole, though never considered as such before.

But as I have the aid of very correct views of the most important parts of the façade, to the accuracy and fidelity of which I have already obtained your testimony—I shall venture to proceed, for I am anxious to bring into notice the most complete exposure of the internal structure of a district, that I have seen or read of; as there is little likelihood that any other person will enjoy the opportunities which I have had for so many years, of exploring this interesting part of our coast, through a turbulent sea, and rapid tides.

Description of the Promontory of Bengore, and its Stratification.

THIS promontory commences at the termination of *Bushfoot Strand*, where the coast, the general direction of which for several miles had been due east and west, turns to the north-east, and after being cut into several semi-circular bays, deflects to the S. S. E. and near the old castle of *Dunseverick*, resumes its former rectilineal and nearly eastern direction.

The promontory occupies the interval between *Dunseverick*, and the *Black Rock*, at the end of *Bushfoot Strand*, about four English miles; the façades commence at *Black Rock*, and increase in height until we reach *Pleskin*, where the perpendicular part at the summit is 170 feet, and the precipitous part from the bottom of the pillars to the sea 200. As we proceed on from *Pleskin* to *Dunseverick*, the height gradually abates, and is finally reduced to about 100 feet.

In this whole space, wherever the precipice is accurately perpendicular, the several strata are easily distinguished from each other, but where the slightest obliquity prevails, a grassy covering is formed that effectually conceals all beneath it; hence the face of the precipice seems much diversified; the columnar strata in some places only exhibiting detached groups of pillars, while in others they form extensive colonnades.

I shall now state the appearances as we approach, and coast the promontory from the westward, noticing in this first view of the precipice, every thing that may be considered as general, and reserving (as I did with you) for my return in the contrary direction, a detailed account of the strata taken separately.

The first circumstance, that occurs to the attentive observer on his approach, is, that although both the promontory itself, and the strata composing it, ascend to the northward, yet it is not in the same angle, the strata being more inclined to the horizon than the line tracing the surface of the promontory, a fact which I shall account for afterwards.

From the *Black Rock* to the *Giant's Causeway* (about a mile) the materials, and their arrangement, are similar to those of the coast to the westward, viz. strata of table basalt, generally separated by thinner strata of a reddish substance.

At the *Giant's Causeway* a new arrangement commences, one of the little systems I have mentioned in other memoirs, by the aggregate of which our coast is formed; nature having changed her materials, or their disposition, or both, every two or three miles. To the system of strata comprehended between the *Giant's Causeway* and *Dunseverick* I now limit myself, as all the strata composing it emerge between these two points.

As we proceed along the coast from the *Giant's Causeway* eastward, we perceive the whole mass of strata ascend gradually, culminate at the northern point of the promontory, and then descend more rapidly, as the land falls away to the south-east, until having traced them across the face of the precipice we see them immerge separately at and beyond *Portmoon Whyn Dykes*.

The western side of the promontory is cut down perpendicularly, by eleven *Whyn Dykes*; the intervals between them are unequal, but they all reach from the top of the precipice to the water, out of which some of them again emerge in considerable fragments; they are all constructed of horizontal prisms, which are strongly contrasted with the vertical pillars of the strata through which they pass.

One of the dykes at *Port Cooan*, on *Bengore*, half a mile from the *Giant's Causeway*, is very beautiful; an insulated rock about 160 feet high, and 20 in diameter, stands perpendicular in the middle of a small bay; the main body of the rock is similar to the contiguous consolidated masses; but on the east side a singular whyn dyke is joined to it, composed, (as they often are,) of several walls agglutinated together, with wall-like fragments of other parts of the dyke emerging at their base; the solid mass of dyke is seen cutting down the precipice to the southward at 150 yards distance.

Depressions of the Strata.

Soon after we have passed the last of our whyn dykes at *Port Spagna*, (a name derived from a vessel belonging to the Spanish Armada having been driven ashore in that Creek), we discover a new and curious circumstance, viz. that the western half of the promontory has sunk or subsided between thirty and forty feet, without the slightest concussion or derangement of the parallelism of the strata.

Two other depressions appear as we proceed onwards, one at *Portmoon*, and the other at the angle where the promontory begins to project from the rectilineal coast; these however are far less considerable in thickness than the preceding, neither of them exceeding five feet.

Such depressions occur at the collieries near *Ballycastle*, and generally on one side of a whyn dyke. We have also at *Seaport*, two miles west from the *Giant's Causeway*, a dyke, oblique and undulating, with a depression of the strata of about four feet on one side; but on *Bengore* promontory our dykes are unaccompanied by depressions of the strata, and where we havedepressions, we do not find a trace of a dyke.

The portions of this extensive façade, which I have selected for explanatory views, are *Portmoon*, in or near which most of the strata emerge, and *Pleskin*, where the strata culminate, each of these views too, exhibits one of our depressions, but in that of *Pleskin*, the first apparent depression is purely an optical effect arising from the position of my friend Major O'Neal, of the 56th, who took his view from the water.

Enumeration of the sixteen Strata that compose the Promontory of Bengore, *taken in their regular Order, and counting from above.*

The country immediately to the southward of *Bengore* is like the Promontory itself, a stratified mass, accumulated to the summits of *Craig Park* and *Croaghmore*, the first five hundred and the second seven hundred feet high; but with these strata I have nothing to do, limiting myself to those alone of which the promontory is formed, and which are exhibited in its façades.

The uppermost of these commences near half a mile to the eastward of the angle, where the coast deflecting from its due east and west course, turns to the north-west, and begins to form the promontory.

So far the course of this stratum is to appearance perfectly horizontal, for the strata all ascending to the north, the intersection of their planes with the plane of the sea, must run east and west, that is, in the present case it coincides with the direction of the coast.

But when the coast changes its direction, this coincidence ceases, and the façade (that is the vertical section of the coast) losing its east and west course, its strata must appear to ascend towards the point it turns to, therefore the strata at *Portmoon*, and along the north-east side of the promontory, should ascend obliquely along the façades, as they actually do.

First Stratum, (*m*).

The stratum I commence with forms the whole façade, from its first appearance until it reaches the promontory; it consists of massive pillars rather rude, and about sixty feet long, its course for half a mile (as I have stated) seems horizontal, but on the face of the promontory it ascends, and continues to rise uniformly until it reaches the summit, which it lines as far as *Portmoon*, on the south side of which it loses some of its thickness, then suddenly disappears and vanishes from that façade, receding westward in the form of a stony ridge, and is seen no more.

Second Stratum (*k*).

The stratum upon which the preceding rests, is red as brick, and about nine feet thick; it appears in spots, and patches just above high water mark, so long as the incumbent stratum continues horizontal, but when that rises obliquely, the second ascends with it; it is now completely displayed, and having supported the preceding in its course to the summit, vanishes with it (at *x* in the view of *Portmoon*,) and is seen no more.

These ochreous matters, so common in all basaltic countries, according to Mr. F. St. Fond's opinion, were once pure basalt, but have undergone some chemical process of nature we are unacquainted with, by which their colour has been changed.

Third Stratum, (*i*)

The next stratum is the last of those composing the promontory which appears beyond it; for so long as the first and second continue their horizontal course towards *Bengore*, this third accompanies them, shewing its upper surface between high and low water mark, but when it ascends along with the others across the façades, it displays its whole thickness, above fifty feet.

This stratum is of that variety of basalt, I have on different occasions distinguished by the name irregular prismatic; it resembles the columnar basalt in grain, but differs from it totally in principle of internal construction, for its prisms are small, not articulated, and indifferent as to the position of their axes, which is perpetually changing.

The irregular prismatic basalt accompanies the columnar in most countries, as at *Pont du Baume*, at *Trezza*, at *Bolsena* in the *Sound of Mull*, and at *Staffa*. In *Antrim*, it is very common; and here is a striking resemblance between the rock crowning the celebrated columns at *Staffa*, and a stratum covering a very neat colonnade at *Craigahullur*, near *Portrush.*

This stratum (as is well exhibited in the view of *Portmoon*) is scolloped off irregularly from the point where it becomes superficial (*x*), until it completely disappears at (*x*); a thin stripe of its lower edge alone is ever resumed again.

Fourth Stratum, (*h*).

The next three strata will require only very short descriptions; the fourth is about seven feet thick, entirely columnar, the pillars small, but not neat; they appear very white from a thick covering of *Byssus saxatilis*, which shews a great predilection for this stratum.

Fifth Stratum, (*g*).

This stratum is ochreous, and more of a slate colour than any of the other red strata; as it is friable, it soon acquires a grassy coat, through which it is only in spots that it shews its proper colour; it is about eight feet thick.

Sixth Stratum, (f).

This stratum is composed of rude massive pillars so coarsely formed, that on the least abatement of perpendicularity the columnar form can scarcely be traced. This stratum is about ten feet thick, it forms the vertex of the beautiful conical island *Beanyn Daana*, and is marked in the views (*f*).

These last strata, though they have nothing very remarkable in themselves, nor contribute much to the beauty of the façade; yet they exhibit one of the most important facts I am acquainted with in natural history, and which, when attentively considered, throws much light on the nature of the operations performed upon our globe since its consolidation, and leads us irresistibly to conclusions extraordinary and unexpected.

The fourth, fifth, and sixth strata reach the top of the precipice, and vanish together at the waterfall in the north-west corner of *Portmoon*. When they come to the surface, they turn inland to the westward in long stony ridges; these obstruct the course of the waters in their descent along the inclined plane, formed by the surface of the promontory, and throw them over the precipice, in a cascade highly beautiful after rain.

On the façades to the north-west not a trace of them appears, these being entirely formed by the lower strata, which I have not yet noticed; but at the distance of a mile, at the great depression (already mentioned), the fourth, fifth, and sixth strata, with a narrow stripe of the third, suddenly appear, in their regular posts, their proper order, and with all the characteristic marks peculiar to each separate stratum.

In the interval between the depression at *Pleskin*, and the *Giant's Causeway* (about a mile), these three strata often appear in a desultory way on the summit of the precipice, wherever it is of sufficient height to receive them, always preserving their usual thickness, their characters, and their order; so that a person master of the order I am detailing, as he approaches a rising point of the precipice, can tell its strata, and their order, before he is near enough to distinguish them.

Seventh Stratum, (*d*).

The rude and massive pillars of the sixth stratum pass into the neater, and much longer columns of the seventh, without interrupting the solidity or continuity of the material; exactly as a down held hand appears to separate into fingers. The thickness of this stratum, that is the length of the pillars of which it is formed, is fifty-four feet; it is marked (*d*) in the two views, and in its passage across the face of the precipice, displays more beautiful colonnades than any of the others.

This seventh stratum emerges from the beach immediately behind the south-east point of *Portmoon*, and where it first shews itself in that bay, has its lower edge raised only a few feet above the water; it forms the upper frustum of the larger of the two conical islands, ascends obliquely along the face of *Portmoon*, and continues to rise until it composes the upper range in the beautiful façade, properly called *Bengore Head*. This is probably the most magnificent of all, its convexity towards the sea producing a fine effect. The lower edge of this stratum, that is the line forming the base of its pillars, has here, as at *Pleskin*, attained the height of three hundred feet above the water.

This seventh stratum, like those above it, also suffers an interruption; for after having exhibited itself to such great advantage at *Bengore*, the extreme northern point of the promontory lowers, and this stratum disappears for about one-third of a mile; as the promontory rises, it is resumed again in great beauty at *Pleskin*, and is interrupted no more; we scarcely ever lose sight of it until we reach *Port Noffer* (the next bay to the Causeway); here, for want of perpendicularity it is little seen, and is finally lost over the causeway, we know not well how.

Eighth Stratum, (*c*).

The next stratum is of the same variety of basalt with the third, that is, irregular prismatic; it is fifty-four feet thick, and in the views distinguished by the letter (*c*): where it emerges at the south east corner of *Portmoon*, it is quite accessible by land, and affords the best opportunity I know for examining this species of basalt, as it is there very neat.

There is little more of this stratum seen in the façade of *Portmoon* for want of perpendicularity, but it forms the lower frustum of the great conical island *Beanyn Daana*, and the whole of the smaller, except the base; it is well displayed over the remainder of the precipice, it forms the intermediate stratum between the magnificent colonnades at both *Bengore* and *Pleskin*, and finally is lost just over the *Giant's Causeway*. Large globular fragments have fallen from it, and are scattered about the causeway.

Ninth Stratum, (*b*).

This stratum is forty-four feet thick, that being the exact length of the neat pillars composing it; at its emersion it forms the bases of the two conical islands in *Portmoon*, and is no more seen in that bay, but immediately to the northward it begins to shew itself in colonnades and groups, some of them resembling castles and towers.

It ascends along the precipice obliquely, like those above it, forms the lower range at *Bengore* and *Pleskin*, from which last it dips to the westward regularly, composes the group at *Port Noffer*, called the *Organs*, seen from the causeway, and finally at its immersion, or intersection with the plane of the sea, it forms the beautiful assemblage of neat pillars, so long distinguished by the name of the *Giant's Causeway*.

At these two intersections, each of them accessible by land and water, the prisms exactly resemble each other in grain, size, and neatness; the interval between them is full two miles, through great part of which this stratum is displayed at different heights; it culminates between *Pleskin* and *Bengore*, with its lower edge more than two hundred feet above the water.

We see now what a diminutive portion of our vast basaltic mass has, until lately, monopolized the attention of the curious; and even after it was discovered that we had many other, and much finer collections of pillars on the same promontory, it never occurred, to those who were preparing to give accounts of them to the public, to examine whether these were mere desultory groups, or detached parts of a grand and regular whole, which a more comprehensive view of the subject would soon have laid open to them,

Tenth Stratum. (*a*)

The stratum upon which the pillars of the preceding rest, is ochreous, red as minium, and about twenty feet thick; it is scarcely seen at *Portmoon*, a patch alone of its surface being distinguishable under water at low tide; but immediately to the northward it shews itself, and from its bright colour makes a conspicuous figure across the face of the precipice in a course of more than a mile and half; its last appearance to the westward is at *Rovinvalley*, the opposite point of the bay from the *Giant's Causeway*, from which we have a good view of it. The final dip and immersion of this tenth stratum, as well as its emersion, are lost for want of perpendicularity.

The six remaining strata are all similar in material, but differing much from each other in thickness; they are all of that description called tabular basalt, sometimes shewing a faint disposition to assume a columnar form at their edges, and always separated from each other by ochreous layers.

These six strata are not so perfectly distinct as those above them, for sometimes we think we can count seven, and again not more than five; nor does each of these preserve the same thickness through their whole extent, for they are deeper towards the northern point, where they culminate, forming by themselves a perpendicular façade near two hundred feet high, but they grow thinner as they recede from this centre.

The jets of black rock in the view of *Portmoon*, are the emersions of these strata; their last appearance on the west side is at *Rovinvalley*, where they strongly display the inclination of their strata, (the same with all the rest) to those approaching from the westward; their final immersion is lost for want of perpendicularity.

I shall now proceed to select from the great mass of facts that are exhibited on the face of *Bengore* promontory, and occur in the contiguous basaltic country, such as seem applicable to geological questions, and likely to throw light on such subjects.

Facts applicable to geological Questions.

1. Every stratum preserves accurately, or very nearly, the same thickness through its whole extent, with very few exceptions.

2. The upper and lower surface of each stratum preserve an exact parallelism, so long as they are covered by another stratum; but when any stratum becomes the superficial one, its upper surface is scolloped, or sloped away irregularly, while the plane forming its base continues steady, and rectilineal; but the parallelism of its planes is resumed as soon as another stratum is placed over it.

3. The superficial lines bounding the summit of our façades, and our surface itself, are unconnected with, and unaffected by, the arrangement of the strata below them.

4. Nature, in the formation of her arrangements, has never acted upon an extensive scale in our basaltic area, (at least on its northern side, where our continuous precipices enable us to determine the point with precision,) but changes her materials, or her arrangement, or both, every two or three miles, and often at much smaller intervals.

5. Wherever there is a change of material, as from one stratum to another in a vertical line; or where the change is in a horizontal direction by the introduction of a new system; or where a whyn dyke cuts through an accumulation of

strata; in all these cases the change is always *per saltum* and never *per gradus*, the lines of demarcation always distinct, and well defined; yet the different materials pass into each other without interrupting the solidity and continuity of the whole mass.

6. The façades on our coast are formed as it were by vertical planes, cutting down, occasionally, the accumulations of our strata; the upper part of these façades is generally perpendicular, the lower steep and precipitous.

7. The bases of our precipices commonly extend a considerable way into the sea; between the water and the foot of the precipice, (and especially near the latter) there is frequently exhibited the wildest and most irregular scene of confusion, by careless observers supposed to be formed by the ruins of the precipice above, which have fallen down; such, no doubt, was Mr. WHITEHURST's idea, when he describes one of these scenes as "an awful wreck of the terraqueous globe."

But a more attentive observer will soon discover that these capricious irregularities, whether in the form of rude cones, as at *Beanyn Daana*, and the west side of *Pleskin;* or towers, as at the dyke of *Port Cooan* and *Castro Levit*, at the foot of *Magilligan* façade, even spires and obelisks, as to the westward of *Kenbaan*, and at the *Bull of Rathlin;* yet all of these once formed part of the original mass of coast, stratified like it, and their strata still correspond in material and inclination with those in the contiguous precipice.

8. These vertical sections or abruptions of our strata are by no means confined to the steeps that line our coast; the remaining boundary of our basaltic area has several of them

equally grand; and similar abruptions, or sections (though not so deep) are scattered over a great part of our area, and especially on the ridges of our hills and mountains which are cut down in many places like a stair, by the sudden abruption of the basaltic stratum.

9. Wherever the strata are thus suddenly cut off, whether it be a mass of accumulated strata as in the façades on our coast, or solitary strata in the interior; the materials on one side of the abruption are completely carried away, without a fragment being left behind, while on its other side the untouched stratum remains intire and undisturbed.

I shall not proceed to apply these facts to support, or invalidate, any of the numerous theories which have given rise to so much controversy, in which I myself (as you know) have borne some part; I shall look to nature alone, without much reference to opinions, and shall endeavour to trace, by the marks she has left behind her, some of the grand operations she once executed on the surface of our globe.

Varro divided the time elapsed since the beginning of the world into three portions, which he distinguished by the names, *prolepticum*, *fabulosum*, and *historicum*.

The first comprehended the period of absolute darkness; in the second some faint lights were thrown upon the history of its events, by fable and tradition; in the third, the historian had the common aids from which history is usually compiled.

The natural history of the world seems to admit of a corresponding division. In the first I include the formation of our strata, their induration, their derangement from the horizontal position in which they seem originally to have been placed' and the operation of cutting them down by so many whyn-dykes.

In the second division, corresponding to VARRO's *fabulosum*, I comprehend the operations performed upon our globe, posterior to its final consolidation, and antecedent to all history or tradition; operations therefore that can be established by the visible effects alone which still exist, written in strong characters.

The third division contains the period since we acquired some knowledge of natural history, became acquainted with causes and effects, and able to trace the connection between them.

With the operations performed in the first division (corresponding with VARRO's *prolepticum*) modern theorists assume that they are well acquainted, able to account for every appearance, and to detail the whole process of original formation. I however shall decline noticing these early processes of nature, and limit myself to the second division of natural history, hoping from the prominent features of my country that remain still undefaced, and from its curious facts, to trace and demonstrate the great effects that have been produced upon our surface; and though I do not presume to advance farther, I perhaps may assist in clearing the way for future naturalists, and by establishing effects, encourage them to proceed to causes, and help them to discover the powers and agents by which these grand operations have been executed.

Inquiry into the Formation of our perpendicular Façades.

It is natural that the first great operation we proceed to investigate, should be the formation of our magnificent façades, one of which is the principal subject of this memoir.

The line of coast that bounds our basaltic area on its north side, extends about twenty-five Irish miles, in which course the precipices are nearly continuous, and more than one half of them absolutely perpendicular for a great part of their stupendous height. The operation by which they were cut off so abruptly, and left with a formidable aspect towering over our coast, is the one we inquire into.

That these bold precipices once projected farther in many places is easily demonstrated; at *Beanyn Daana*, and at the *Chimney*, the columnar construction was obviously once carried much farther out.

At the *Milestone*, *Portcooan*, and *Portnabau*, the fragments of dykes extend far beyond the face of the precipice.

These same facts, together with the projecting base, shew that these sudden abruptions were not formed by the subsiding, and sinking of one part, leaving the remainder in its place: still less by any violent revolution, or convulsion; as the stratification has not sustained the slightest shock either above, or below the façade.

The formation of our abrupt coast, has been ascribed to the action of the sea beating violently against it, washing away the lower parts, and leaving a perpendicular façade standing; as we often see on the banks of rapid and encroaching rivers.

A cool examination of our precipices will soon prove that our façades could not have been so formed, for we always find them on the highest part of the cliff, and receding from the water, which could be instrumental in bringing down the materials from above, only by washing, and so wearing away the bases of the steepest parts; but the elevations of these bases are utterly irreconcileable to this supposition; for instance, the base of *Pleskin* façade is two hundred feet above the present level of the sea, that of *Fairhead* three hundred; now had the sea ever risen to either height, it would have submerged a great part of *Ireland*, and none of the neighbouring country (whatever its level may be) bears the least resemblance to alluvial ground, nor shews any mark of having been once covered by the sea.

The next argument is still more conclusive; the boundary of our basaltic area on its north side, is for twenty-five miles also the confine of sea and land; so far it is natural to ascribe its features, and characteristic marks, to the action of the powerful element that beats against it. But when that precipitous boundary ceases to be the confine of sea and land, turns southward towards the interior, and becomes the line of demarcation between the basaltic and schistose country on the west, it still preserves its former character; that is, of a range or ridge of very high land, steep to the exterior, and sometimes cut down vertically into façades, like its northern part that lines the shore.

Thus *Magilligan Rock*, (four miles inland) is not inferior in magnificence to any of our façades on the coast, its perpendicular section is one hundred and seventy feet, and this continuous for a mile; the façades at *Bienbraddock* are nine miles farther inland, and those of *Monyneeny* thirteen; while the base of the lowest of these perpendicular precipices is elevated 1400 feet above the sea.

The same style prevails on the east side of our basaltic area, after its boundary ceases to be the confine of sea and land; for the limestone façades at *Garron Point*, (considerably above the level of the sea) exactly resemble those of *Dunluce* and *Kenbaan* at the water edge; and *Cave Hill* (several miles from the sea, and nearly one from the shallow estuary of *Belfast*,) exhibits basaltic façades at the height of one thousand feet, precisely similar, and little inferior to those of *Magilligan*.

The exact resemblance between our inland façades (on the east and west sides of our area) to those on the shore, proves them to be all effects from the same cause, and that our accumulated strata have in all these similar instances been cut down vertically by the same agent, and that this agent was not the sea.

Nor has this powerful agent confined its operations to our coast, or to the periphery of our basaltic area; we can trace it over its whole surface; we find throughout its interior, similar, though very diminutive abruptions, executed precisely in the same manner, that is, strata cut across by a long vertical façade, their planes on the upper side perfectly undisturbed, while on the lower side all the materials of which that part of the stratum was once composed are completely carried off.—(See 6th fact.)

We are now unavoidably led into a discussion of a question which has at all times occupied the attention of naturalists.

Whence arise the Inequalities with which the Surface of the Earth is so exceedingly diversified?

I shall not attempt to encounter this question generally, nor to extend my enquiries beyond the limits I have prescribed to myself; but I shall try whether the curious facts so profusely exhibited over our basaltic area, throw any light upon the formation of our own inequalities, or lead us a step towards the discovery of the operations by which such stupendous effects have been produced.

Some to escape the difficulties in which this question is involved, ascribe our inequalities to original formation; as if the world had come from the hand of the Creator with the variegated surface which now contributes so much to its beauty; but the frequent interruptions, and resumptions of the strata in our area, with the perfect resemblance of the corresponding parts, however great the interval by which they are separated, can scarcely leave a doubt that these strata were at first continuous; of course, the figure of our surface at that time must have depended on the original positions and inclinations of these strata, which, as appears by the 3d fact, are now unconnected with the superficial line, the figure of which is governed by their abruptions and removals alone.

Naturalists have differed much in opinion as to the direction in which the causes acted that produced the inequalities on the surface of our globe; some referring us to the bowels of the earth as to the scene of action; while others assert that the operations were performed upon the surface itself.

But the slightest inspection of our façades will at once prove that the first hypothesis cannot be correct; for obliquity of direction must have been the result of a disturbing cause acting from below; whereas parallelism and a steady rectilineal course distinguish the basaltic arrangements of which I have been treating.

We have, it is true, occasional depressions of our strata, where they obviously have subsided, and no doubt from a failure of support below; but in no instance that I have met with, in our area, are these attended by the slightest concussion; the permanent and subsided parts, with us still preserve their parallelism, and the continuity of their material; whence it is probable this event took place previous to the induration of the strata, and of course antecedent to the period to which I limit myself.

Buffon ascribes our superficial inequalities to the agitation of the waters while they covered our earth, and argues from the resemblance these inequalities bear to the waves of the sea; a resemblance I cannot trace in any country which I have observed; nor could our sudden and perpendicular abruptions, ever have been produced by any agitation of the waters.

Professor Playfair considers rivers as having formed not only the beds, or channels in which they flow, but also the whole of the vallies through which they run, and in general all the inequalities of our surface; but an attentive observer, tracing the course of any of our most rapid rivers, would soon perceive that the quantity of its depredations have been comparatively insignificant, and that they can be determined with precision; the river has no doubt in several places extended itself considerably on both sides, but in the intermediate space between the remotest boundaries it ever reached, it levels, instead of raising inequalities.

The same result I apprehend would follow from the operations of another agent, which theorists are in the habit of calling in to their aid, when they cannot find some certain material, which from their theory we had reason to expect; they then tell us it has been carried off, and lost in the suite of degradations and decompositions.

But decay and decomposition, instead of creating inequalities would produce a contrary effect, and deface those actually existing; they would gradually abate the height of our perpendicular façades, and increase the green steep at their bases by the accumulation of the crumbling and mouldering materials from above; while the more diminutive façades formed by the abruptions of single strata scattered over the face of our area, and forming its most characteristic feature, would in time (as many are already) be converted into steep acclivities covered with verdure.

Such are the principal causes to which the inequalities of our surface have been generally ascribed. Previous to our deciding finally upon their insufficiency, it may be proper to enumerate a few of those inequalities, where the deviation of our present surface, from the form it probably had originally, is not only striking, but where also the concomitant circumstances afford demonstration, that some great operation has once taken place there.

Thus, by making ourselves acquainted with effects, we shall be better qualified to investigate causes; and if those effects shall appear to be beyond the powers of such natural agents as we are already acquainted with, we shall be justified in admitting the performance of operations to which we have seen nothing similar; and also in admitting the former existence of powers of far superior energy to any we have ever known in action.

Enumeration of some remarkable Inequalities in the Surface of our basaltic Area, produced since the Consolidation of its Strata.

That we may better understand the facts I am proceeding to state, I shall assume (in the style of the mathematicians *puta factum*) previous to demonstration, that the planes of our uniform, rectilineal strata, however interrupted we may now find them, were once continuous.

Upon this supposition, the valley of the *Mayola*, between the stratified summits of *Seafin* and *Slievegallon*, is an excavation 1700 feet deep, and three miles wide, of which the whole materials have been completely carried off.

To the northward of this excavation, between *Seafin* and *Carntogher*, the continuous accumulated strata of basalt are interrupted, and taken away quite down to the schistose substratum; while the untouched summits of the contiguous mountains, *Carntogher*, *Seafin*, and *Monyneeny*, are still stratified basalt.

On the eastern side of our area, immediately to the southward of *Kello* and *Connor*, a similar operation has been performed, attended by still more extraordinary circumstances.

We here find a district near four miles in diameter, called the *Sandy Braes;* over this whole space the basaltic stratification has been carried off, and the operation has reached deep into a very singular substratum; a reddish breccia, by some mineralogists called a porphyry, the mass friable, but the component angular particles of extreme hardness.

The hills, of which this little district is full, are every one perfect segments of spheres, while the loftier basaltic hills that surround it preserve their characteristic form, to wit, a gradual acclivity on one side, with a steep abruption on the other.

As we sail along our northern shore we discover another great chasm or interruption of our strata, which also cuts deep into the substrata: for on the west side of *Ballycastle* pier, the bold basaltic precipices suddenly disappear, and at a lower level disclose the substratum, which appears to be an alternation of sand-stone, and coal, sometimes with bituminous schistus.

A mile or two to the eastward the abrupt precipice is resumed, and a basaltic stratum again occupies its summit on to *Fairhead*, with the same angle of inclination in which it was disposed along our whole coast, that is, a slight ascent to the north.

Traces of similar operations and abruptions are to be found over our whole area, but the preceding are sufficient to make us acquainted with the style of these interruptions of our strata; of course it is time to proceed to the suspended demonstration, that our strata, so interrupted, were once continuous, notwithstanding the magnitude of the interval by which the corresponding parts are now separated.

Proofs that our now interrupted Strata were once continuous.

We must now turn back to the façades of *Bengore*, where the strata themselves, and all the circumstances attending them, are so happily displayed, as to throw great light on the subject, and to lead us analogically, step by step, to the conclusion we seek for.

Let us examine and trace the summit of the precipice for a mile immediately eastward from the *Giant's Causeway*, and we shall find a frequent interruption and resumption of the fourth, fifth, and sixth strata, at the shortest intervals, the interruption not always reaching to the lowest of the three, which in that case remains continuous: so far simple inspection removes all doubt, that each of these strata was once continuous as far as the great depression to the west of *Pleskin*.

Here indeed the interruption becomes considerable, not less than a mile; but when we find at *Portmoon* a succession of three strata with the same inclination, in the same order, of the same thickness each, and with the same strong characteristic marks that distinguished the three interrupted, at the depression; above all, when we find the strata they rest upon continuous (at least with very trifling interruptions) for the same extent; I think we can scarcely entertain a doubt that this interval between the corresponding parts, though so much greater than any of the preceding, is, like them, but an interruption, and that these strata were once continuous from the depression to *Portmoon*.

The same style of induction would establish the quondam continuity of all the strata in the face of *Bengore* promontory, for here the strata are so distinctly marked that we know each of them when we find it again after any interruption.

In the rest of our precipices and façades, the similarity of the strata deprives us of this advantage; yet in their smaller interruptions, the eye, by tracing the rectilineal course of the strata, and so connecting the separated parts, can establish their former continuity: while in the greater intervals we must rest our proof on analogy alone.

That we may be entitled to carry this style of induction into the interior of our basaltic area, and to apply the same reasoning to enable us to form a similar decision upon the more stupendous interruptions of our strata, which I have already enumerated, it becomes necessary to explain the geological construction of our area,—the strata of which it is formed—their arrangement—and their inclinations.

An extensive limestone stratum, white as chalk, and about two hundred feet thick, seems to form the base of the whole district I limit myself to: upon this, accumulations of rectilineal and parallel basaltic strata, are heaped up to most unequal heights.

This great calcareous stratum seems not to be an accurate plane, but rather to resemble a bason, as every where at its periphery it dips to the interior; yet the plane of its section has a slight ascent to the southward: a recollection of these circumstances will enable us to account for every appearance this stratum exhibits, as it happens to be disclosed to us; and by the converse, an attention to these appearances will enable us accurately to determine the position of the stratum.

This stratum, from *Ballycastle* to *Solomon's Porch*, (about twenty-five miles,) keeps very nearly the level of the sea, often indeed sinking below the surface, but never raising its lower edge above it; but when at *Solomon's Porch*, the boundary of our basaltic area begins to deflect to the south-west, and then to the south, the ascent of the stratum to the southward begins to operate, and we perceive the dotted line of its quarries gradually to rise along the face of the mountain from the shore to *Monyneeny* and *Seafin*, where it has attained the height of 1500 feet: it is true, the actual stratum has not been opened at these two great elevations, but the white rubble immediately below the basaltic façade proves incontestably that it is close at hand.

An accumulation of basaltic strata, had in this southern course, as well as on the northern shore, covered the limestone up to the summits of the hills or mountains.

I have already stated how the ridge of mountain is suddenly interrupted by the valley of the *Mayola*, from 1600 to 1700 feet deep, but if we look to the southward, in the rectilineal course of the strata (the positions of which we have been able to ascertain with so much accuracy), we shall find near the summit of the mountain *Slievegallon* a similar white limestone stratum crowned with basalt, cutting it in the very direction the former ought to have reached it, that is perhaps two hundred feet higher, the ascent of the strata to the southward having elevated their planes so much in a distance of four miles, the probable interval between the summits of these mountains.

We are now to decide whether this calcareous and basaltic fragment, on the summit of *Slievegallon* mountain, be the last remnant of the old arrangement we have been tracing, and ascertaining with so much precision, for seventeen or eighteen miles from the sea, and twenty-five miles along the coast, but now interrupted by the valley of the *Mayola*, like our former more diminutive interruptions, and also like them resumed at the next elevation, in the same rectilineal course, the strata preserving the same order, and the same characteristic marks. Or whether these strata, appearing on the summit of *Slievegallon*, be the commencement of a new arrangement, abandoned by nature as soon as begun : which is highly improbable, for neither limestone nor basalt are to be found on the mountain except in this solitary hummock.

We might, by a minute attention to the inclinations, and arrangements, of the strata contiguous to the other interruptions I have enumerated, prove in like manner that the basaltic masses crowning the summits of the surrounding hills and mountains, are merely the remnants of strata once extensive and continuous, but interrupted and carried off, as in the preceding case, by the same powerful agent.

The more diminutive inequalities scattered over the whole surface of our area, and always produced by interruptions of the strata, would still more easily admit the appplication of the same reasoning, from the contiguity of their abrupted parts; but the detail would be tedious; those who wish to pursue the subject farther must come to the scene themselves.

Materials completely carried off.

A circumstance perhaps still more extraordinary, is the complete removal of all the materials that once filled up the intervals between the abrupted parts of these strata; I have stated in my 9th fact, that the materials that had formerly composed the projecting parts of our northern façades, and precipices, had totally disappeared.

The removed parts of the limestone stratum on the west side of our area have shared the same fate, for where the chain of mountains extending from *Magilligan Rock* to *Bienbraddock*, is interrupted by vallies at *Stradreagh*, *Drumrommer*, and *Ballyness*, it is obvious that the limestone stratum was once continuous to the high points where it shews itself on *Keady*, and the mountains on each side; its thickness too, wherever we can try it, is very great; yet this stratum, which in its entire state must have spread like a roof far above the present surface of these valleys (which are now sunk deep into the schistose substratum) has not left a particle of its *debris* behind, nor is a single lump of white limestone to be found, until we come to the quarries, that is, to the edge of the solid, untouched stratum.

Conclusions.

The conclusions that unavoidably follow, from the consideration of these facts are,

That the hills and mountains, in the district I have been describing, were not raised up or formed as they now stand, but that they are the undisturbed remains of strata that were left behind, when stupendous operations carried away the parts that were once contiguous to them.

That the inequalities of this surface were all produced by causes acting from above, and carrying off whatever they touched, without in the least disturbing what was left behind.

Additional Evidences. Basaltic Hummocks.[*]

The arguments on which I have founded my opinions have hitherto been all taken from the hollows in our surface, and the interruptions in our strata, both which, the concomitant circumstances have led me to consider as so many excavations; but the lofty elevations, and the abrupt prominencies rising suddenly from our surface, when minutely examined, lead us irresistibly to the very same conclusion.

When you and I examined together the line of our northern façades, we studiously sought for the points where nature had made any change in her materials or their arrangement, hoping that at the junctions of these little systems, we should find some facts that would throw light on the subject; but we generally failed; want of perpendicularity, or other local circumstances, impeding us at the most interesting points.

On the present occasion she has adopted an opposite line of conduct, and in many of the steps she has taken, obtrudes upon us demonstration of what she has done.

Whoever has attended to the exertions of man, when employed in altering our present surface, either by levelling heights, or by making excavations, must have observed that it is the practice of the workmen to leave small, cylindrical portions standing, for the purpose of determining the height of the old surface, and thereby ascertaining the quantity of materials removed.

To these may be compared the stratified basaltic hummocks so profusely scattered over our area, and which seem to shew how high our quondam surface once reached.

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[*] Navigators use the word hummock to express circular and elevated mounts, appearing at a distance; I adopt the term from them.

The hummock of *Dunmull*, three miles south-east from *Portrush*, is very beautiful, it stands on the top of a high ridge, and is a conspicuous object from all parts of the country; it is exactly circular, its flat surface contains an acre, it is completely surrounded by a perpendicular façade about twenty-five feet high, and formed by two strata, a columnar, and an irregular prismatic, resting upon it.

From this elevated station, where I had the pleasure of accompanying you, I shewed you at six or seven miles distance to the westward, among the *Derry* mountains, the still loftier hummocks of *Altabrian* and *Sconce*, hemispherical in form, composed of but one stratum each, while their swelling-out bases displayed accumulations of many more: and also near those the hummock of *Fermayle*, resembling *Dunmull*, but much larger, and also, surrounded by a façade composed of two strata.

I shewed you also at twenty miles distance to the south-east, the gigantic *Slemish*, one of our basaltic hummocks, magnified (as it were) into a lofty and insulated mountain, completely stratified from its base to its flat summit.

I shewed you likewise from the bottom of its ridge, the neat but diminutive hummock, called the *Rock of Clogher*, above *Bushmills*. As our time was precious, you took my word for its stratification being precisely similar to that of *Dunmull*.

There are many other basaltic hummocks scattered over the surface of our area, all of them either stratified or portions of strata; two of the most remarkable are the hill of *Knock Loughran*, near *Maghera*, and a tall hummock (whose name I forget) a mile eastward from *Lisanoure*.

We meet still more frequently an imperfect style of hummock, a semi-circular façade on one side, while on the other it slopes away gradually with the dip of the strata, as if the operation had been interrupted before it was carried quite round; the most remarkable of these are *Ballystrone*, in *Derry*, and *Croaghmore*, in *Antrim*, both visible from *Dunmull*.

Regular stratifications on the summits of hills and mountains, have been long a stumbling block to theorists; the historian of the French Academy, for the year 1716, obviously ascribing the superficial inequalities of the earth, (like many others) to causes acting from below, and perceiving how incompatible such assemblages of strata were to his theory, thinks it safer to doubt their existence, as they could not have been formed, he says "unless the masses of the mountains "were elevated in the direction of an axis perpendicular to the "horizon: *ce que n'a pu être que très rare.*"

But as these stratified mounts are in our area by no means uncommon, they lay us under the necessity of suggesting another alternative similar to those we have already stated.

Were these isolated hummocks originally formed as they now stand, (solitary and separate from each other) one by one; or, are they the last remaining portions of a vast consolidated mass, of which the intermediate and connecting strata have been carried off by causes with which we are unacquainted?

To be able satisfactorily to resolve this alternative, it becomes necessary to take a careful view of the contiguous countries, and to try whether their construction, and the arrangement of their strata, will throw any light upon the subject.

When we examine the assemblage of hummocks above *Knockmult*, that is, *Sconce*, *Fermoyle*, and *Altabrian*, we find their materials and stratification, precisely similar to that of the country below them to the eastward, where the abruptions of the strata are displayed in long stony ridges ;—to the south, the abruptions on the summit of *Keady* mountain discover the same similarity; and to the north-west the grand façade of *Magilligan Rock*, three miles distant, displays an accumulation of exactly the same sort of strata consolidated into an enormous mass.

The hummock of *Dunmull* is formed of two very particular strata, a columnar, and an irregular prismatic; but I shewed you, a mile to the northward, at the façades and quarries of *Islamore* and *Craigahuller*, at the base of the hill, that the whole ridge, on the summit of which *Dunmull* is placed, was a consolidated mass, formed by alternate strata of the same description: and that the arrangement of the whole country below, and adjacent, was precisely the same with that of the hummock of *Clogher*, I proved to you at the curious opening of the strata at *Bushmills Bridge*, and in the façades at the *Giant's Causeway*.

After these proofs that so many (and I might proceed to the rest) of our detached hummocks, are in their construction and materials, similar to, and connected with, the main consolidated masses of which our country is formed, I think it will scarcely be asserted that these hummocks were originally formed, solitary and separate as they now stand; but rather that they were once parts of that vast whole, and left behind in their present form, upon the removal of the contiguous portions of their strata, by some powerful agent, of whose operations and modes of acting, we have hitherto obtained little knowledge.

The highest point on the façade of *Cave Hill* is called *M'Art's Castle*, and appears to be a solitary fragment of a stratum, precisely similar to those below it, and obviously once extended like them.

The irregularity of the summit of *Fairhead*, plainly shews that its gigantic columns once reached higher.

And in the façade of *Magilligan*, the highest of all, a few desultory patches of an upper stratum (no doubt once perfect and continuous) are to be traced along its summit.

Our mountains themselves seem to shew clearly that they were once higher; the top of *Magilligan* mountain is an extensive plain, over which a wandering stratum is interrupted and resumed at intervals for a great way.

At the highest part of *Donald's Hill*, over the valley of *Glenuller*, we find a hummock; also at the termination of the ridge, at its highest part over the valley of *Mayola*, similar hummocks.

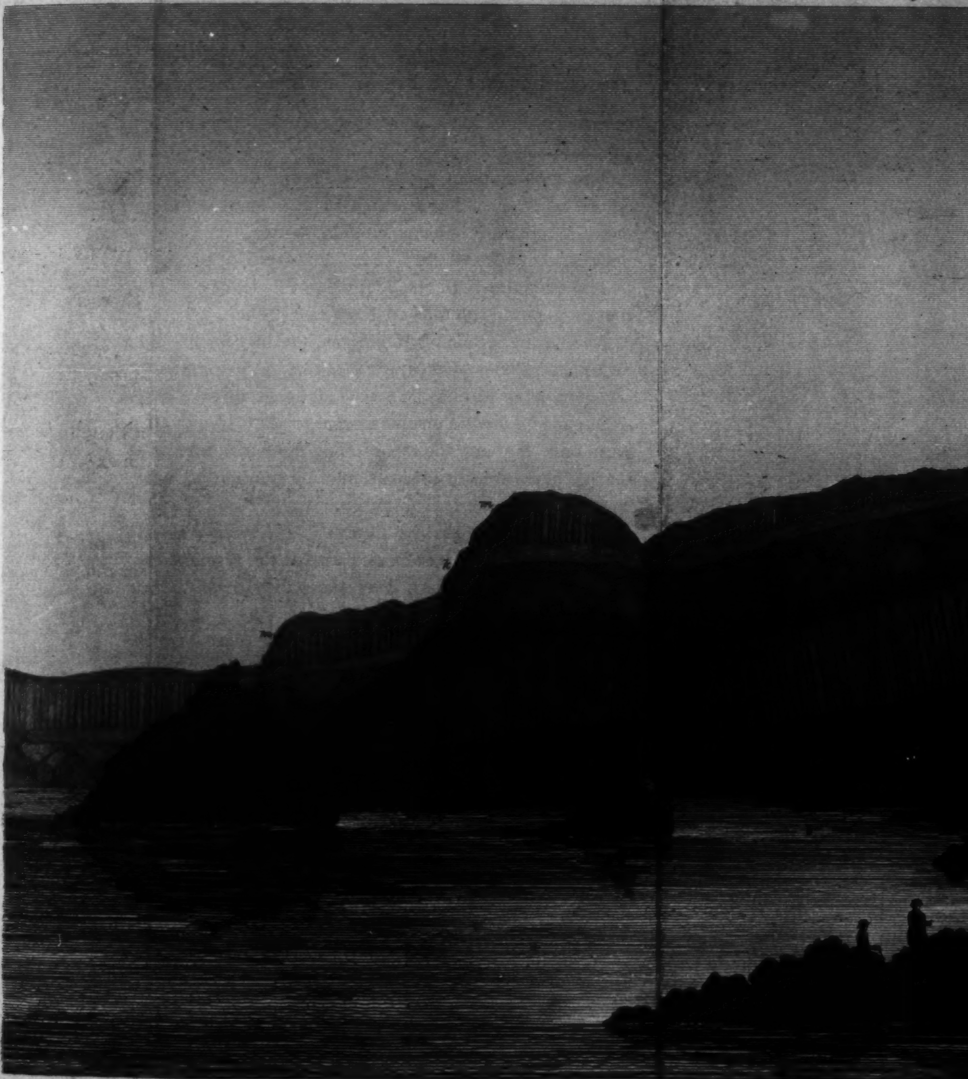
I have the honour to be,

SIR,

Your obedient, humble Servant,

W. RICHARDSON.

Clonfecle, Jan. 2, 1808.



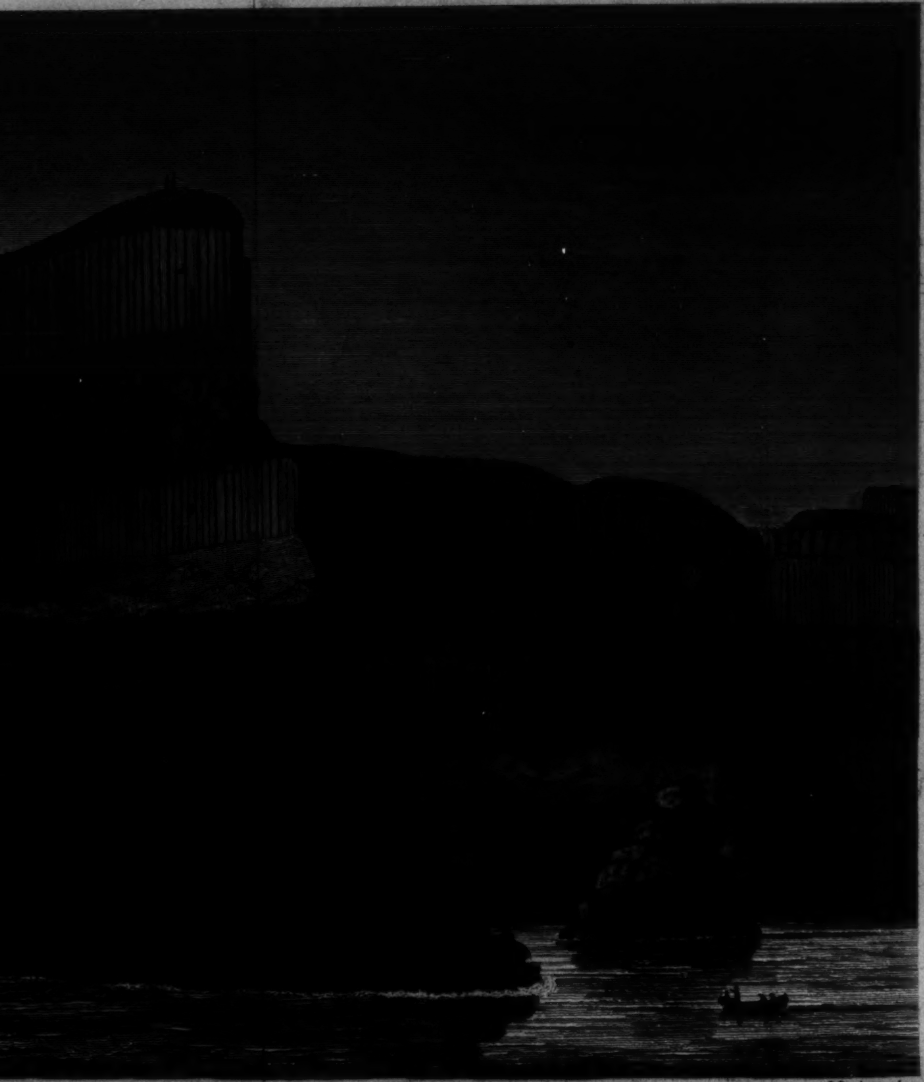
View of Port

J. Basire sculp.

Portmoon.

Robt. O'Neil del.

Philos. Trans. MDCCCVIII. *Plate* VI. *p.* 222.



J. Basire sculp.

XV. *A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the urinary Passages; and on the Effects that are produced upon them, by the internal Use of solvent Medicines, from Mr.* William Brande *to* Everard Home, *Esq. F. R. S.*

Read, May 19, 1808.

Dear Sir,

Having availed myself of the opportunity you procured for me, of making a chemical examination of the calculi contained in the Hunterian Museum, as well as those in your own collection, I herewith send you an account of what I have done.

Should the observations appear to you to throw any new light upon the formation of calculi, I request that you will do me the honour of laying them before the Royal Society.

The collection which I have examined, is not only uncommonly large, but the greater part of the specimens have histories of the case annexed to them.

This circumstance enabled me not only to ascertain the situations in which the calculi were found, but likewise many of the circumstances attendant on their formation.

I have therefore endeavoured to form an arrangement upon these principles, with a view to render the subject more clear and perspicuous.

SECTION I.

Of Calculi formed in the Kidnies, and voided without having afterwards undergone any Change in the urinary Passages.

These have the following properties :

They are of a brownish yellow colour, sometimes of a grayish hue, which seems to arise from a small portion of dry mucus adhering to their surface.

They are entirely soluble in a solution of pure potash, and during their solution, they seldom emit an odour of ammonia.

When heated to dryness, with nitric acid, the residuum is of a fine and permanent red colour.

When exposed to the action of the blow-pipe, they blacken and emit a strong odour of burning animal matter, very different from that of pure uric acid. This arises from a variable proportion of animal matter which they contain, and which occasions the loss in the analysis of these calculi. Its relative quantity is liable to much variation, as may be seen from the following statements.

A calculus from the kidney, weighing seven grains, was dissolved in a solution of pure potash. A quantity of muriatic acid (rather more than sufficient for the saturation of the potash) was added, and the precipitate of uric acid thus obtained weighed when dry 4,5 grains. No other substance, except animal matter, which was evident on attempting to obtain the muriate of potash, could be detected, consequently the composition of this calculus was as follows :

			Grs.
Uric acid	-	-	4.5
Animal matter	-	-	2.5
			7.0

This is the largest proportion of animal matter which I have met with.

A small calculus from the kidney, weighing 3.7 grains, afforded by a like treatment 3.5 grains of uric acid, so that it was nearly a pure specimen of that substance.

The largest calculus of this kind which I have examined weighed seventeen grains; much larger ones have been found, but there is no evidence of their not having remained in the urinary passages for some considerable time. Thus Dr. HEBERDEN mentions one weighing twenty-eight grains.*

It often happens that the ingredients are not united together so as to form a calculus, but are voided in the state of a fine powder, commonly termed sand. This consists either of uric acid, or of the ammoniaco-magnesian phosphate, alone, or with the phosphate of lime.

I am induced to believe that the last mentioned substances, although the production of the kidnies, and held in solution, are never met with in a separate state till the urine has been at rest, and therefore, calculi from the kidnies are never composed of the phosphates.

In a few instances, calculi from the kidnies, composed of oxalate of lime, are voided; but this is a very rare occurrence: of three preserved in the HUNTERIAN Collection, two are extremely small and hard, having an appearance of being made up of several smaller calculi, of a dark brown colour. The third is of the size of a small pea, its surface smooth, and of a gray colour, and not very hard.

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* Comment. on the Hist. and Cure of Diseases, 3d. edit. p. 88.

SECTION II.

Of Calculi which have been retained in the Kidney.

When one or more of the calculi described in the preceding section are detained in the infundibula or pelvis of the kidney, it frequently happens that they increase in that situation to a considerable size.

This increase is of two kinds.

1. Where there is a great disposition to the formation of uric acid, the calculus consists wholly of that substance and animal matter, so as frequently to form a complete cast of the pelvis of the kidney.

2. Where there is less disposition to form uric acid, the external laminæ are composed of the ammoniaco-magnesian phosphate, and phosphate of lime.

In one instance, a small uric calculus had been deposited in the kidney, in such a situation that its upper surface was exposed to a continual stream of urine, upon which beautiful crystals of the triple phosphate had been deposited. It would therefore seem, that under common circumstances, a stream of urine passing over a calculus of uric acid, has a tendency to deposit the phosphate upon it.

SECTION III.

Of Calculi of the urinary Bladder.

Calculi met with in the bladder are of four kinds.

1. Those formed upon nuclei of uric acid, from the kidney.
2. Those formed upon nuclei of oxalate of lime, from the kidney.
3. Those formed upon sand or animal mucus, deposited in the bladder.
4. Those formed upon extraneous bodies introduced into the bladder.

They were arranged under the following divisions.

1. Calculi, which from their external appearance, consist chiefly of uric acid.

These calculi vary in colour from a deep reddish brown, to a pale yellowish brown.

They are either entirely soluble in a solution of pure potash, or nearly so.

During their solution they frequently emit the odour of ammonia.

When acetic acid is added to their alcaline solution, a precipitate possessing the properties of uric acid is obtained.

2. Calculi, composed chiefly of the ammoniaco-magnesian phosphate, or of phosphate of lime, or of mixtures of the two.

These calculi are externally of a whiter appearance than the former.

Some perfectly white, others gray, occasionally exhibiting small prismatic crystals upon their surface; others again soft and friable, a good deal resembling chalk. They are further characterised by their solubility in dilute muriatic acid.

3. Calculi, containing oxalate of lime; commonly called mulberry calculi.

These are distinguished by the difficulty with which they dissolve in dilute acids, by their hardness, and by leaving pure lime, when exposed to the action of the blowpipe.

In the examination of these calculi, I was struck with the small number of those strictly belonging to the first division, having been led, from the account of Fourcroy and Vauquelin,[*] and the experiments of Dr. Pearson,[†] to believe that calculi, composed of pure uric acid, were by no means unfrequent.

The greater number of the calculi examined by the former chemists, are stated to be completely soluble in the fixed alcaline leys; and of three hundred examined by Dr. Pearson, a large proportion is said to consist of uric acid.

The following is a statement of the composition of the different calculi found in the bladder which I have examined.

16 were composed of uric acid.
45 ———————— uric acid with a small relative proportion of the phosphates.
66 ———————— the phosphates, with a relatively small proportion of uric acid.
12 ———————— of the phosphates entirely.
5 ———————— of uric acid, with the phosphates and nuclei of oxalate of lime.
6 ———————— chiefly of oxalate of lime.
———
150

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[*] Annales de Chemie, xxxii. 218.

[†] Philos. Trans. 1798. p. 37.

To injure these calculi as little as possible, they were carefully cut through with a fine saw, and a portion of the whole cut surface removed by a file; in this way all the different ingredients of the calculi were obtained.

In the experiments upon uric calculi from the bladder, I found, in most instances, a far more considerable loss in attempting to obtain their pure uric acid, than in the kidney calculi, which led me to suppose that they contained urea and that the presence of this substance, with some of the salts of urine, and with small portions of the ammoniaco-magnesian phosphate, was the cause of the occasional evolution of ammonia when treated with the fixed alcalies, and of their easy solubility in those substances.

To determine this point a small calculus, weighing twenty-five grains, and of the species commonly supposed to consist of urate of ammonia,* was digested for two hours with water in a very moderate heat. The water which had assumed a pale yellow colour was filtered off, and fresh water added to the residuum three successive times, when it appeared that every thing soluble in that fluid was separated. The insoluble part of the calculus being now carefully dried and weighed, was found to have lost 5.5 grains.

The aqueous solution was evaporated by a gentle heat, nearly to dryness, and a substance was obtained having all the properties of urea, in combination with a small portion of muriate of ammonia, and of the ammoniaco-magnesian phosphate.

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* FOURCROY observes that urate of ammonia is easily detected by its rapid solubility in the fixed alcalies, and the odour of ammonia, which is perceived during its solution.—Vide Thomson's Syst. of Chem. vol. v. p. 691.

Sixty grains of another calculus of a considerable size, supposed from a superficial analysis, to consist of nearly pure urate of ammonia, were digested at a low temperature in one ounce of alcohol. In an hour the alcohol was decanted off, and fresh portions were added successively, as long as it appeared to act upon the calculus, which after having been carefully dried in a temperature below 212°, weighed 54.8. grains, so that 5.2 grains had been taken up by the alcohol.

On evaporating the alcoholic solutions, a substance was obtained having all the properties of urea, with a small portion of saline matter, probably muriate of ammonia, as by the addition of potash, a slight ammoniacal odour was perceptible; its quantity however was too minute for accurate examination.

The remaining portion of the calculus, weighing 54.8 grains, was treated with small portions of acetic acid, by which 6. grains of the ammoniaco-magnesian phosphate were obtained.

The part of the calculus remaining after this treatment, weighing 48.8 grains was perfectly soluble in a solution of pure potash; it emitted no ammoniacal odour when acted upon by the alcali, and possessed the properties of pure uric acid.

The following therefore is the composition of this calculus.

	Grains.
Urea, and muriate of ammonia - -	5.2
Ammoniaco-magnesian phosphate -	6.
Uric acid - - - -	48.8
	60.

From these and many similar experiments upon other calculi, hitherto generally supposed to consist of urate of ammonia, I am induced to believe that the evolution of ammonia depends in all instances upon the decomposition of the ammoniacal salts contained in the calculus, more especially of the ammoniaco-magnesian phosphate, and that no substance which can be called *urate of ammonia*, exists in calculi.

The mulberry calculus (oxalate of lime) I have but rarely met with. In those preserved in the HUNTERIAN Collection, there is a large relative proportion of phosphate of lime, and of uric acid. The purest of them afforded

	Grains.
Oxalate of lime - -	65.
Uric acid - -	16.
Phosphate of lime - -	15.
Loss in animal matter .	4.
	100.

When calculi of the urinary bladder increase to a very large size, they are generally composed of two or even three of the above mentioned varieties, the ammoniaco-magnesian phosphate being situated externally, and in the greatest abundance.

The largest calculus which I have seen, weighed, when recently removed from the bladder, twenty-three ounces and twenty-six grains. It consisted of a large mulberry or oxalate of lime calculus, the nucleus of which was uric acid, surrounded by a considerable quantity of the ammoniaco-magnesian phosphate in a very pure state.

Another very large calculus, weighing fifteen ounces and a half, consisted of a nucleus of uric acid, enveloped in the ammoniaco-magnesian phosphate, not however pure, but intersected by several laminæ of uric acid.

Four distinct substances are extremely rare in calculi; I have seen one in which the uric acid, the ammoniaco-magnesian phosphate, the phosphate of lime, and the oxalate of lime, were all in perfectly separate and distinct layers.

Four calculi, having the following extraneous substances for their nuclei were examined.

1. A common garden pea.
2. A needle.
3. A hazle nut.
4. A part of a common bougie.

In the two first instances, the calculous depositions were of a pale gray colour, inclining to white; soft and friable in their texture, and entirely soluble in muriatic acid.

The composition of the first was as follows;

	Grains.
Phosphate of lime - -	65.
Ammoniaco-magnesian phosphate -	28.
Loss - - - -	7.
	100.

Of the second;

Phosphate of lime - -	45.
Ammoniaco-magnesian phosphate -	38.
Oxalate of lime - - -	12.
Loss - - - - -	5.
	100.*

The deposition of calculous matter upon the bougie was covered with blood, and in very small quantity, the bougie having been removed by an operation soon after it had passed into the bladder. It appeared to consist chiefly of phosphate of lime.

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* It appears that in this case there had been an accidental disposition to the formation of oxalate of lime.

The incrustation upon the hazel nut was also destitute of uric acid.

SECTION IV.

Of Calculi of the Urethra.

All those that were examined had escaped from the bladder while very small, and had afterwards lodged in the membranous part of the urethra, where they had increased in size, and formed a cavity in which they were more or less embedded.

Two of these calculi were broken.

The fragments consisted in one instance, of ammoniaco-magnesian phosphate, and phosphate of lime, with a small portion of uric acid: and in the other the fragments were composed entirely of the ammoniaco-magnesian phosphate.

The third calculus was of a very remarkable appearance; its form being that of a perfect sphere, about half an inch in diameter. It was coated with small but very regular crystals of the triple phosphate in its purest state. On account of the singularity of the form and external appearance of this calculus, it was not sawn through; the nucleus, in all probability, is a small kidney calculus, which lodging in the urethra has become coated with triple phosphate.

SECTION V.

Analysis of Calculi from other Animals.

1. The Horse.

A. *From the kidney.*

A very large calculus, from the kidney of a horse, was composed of

Phosphate of lime	- -	76
Carbonate of lime	- -	22
		98.

B. *From the bladder.*

This calculus was also of a large size, its weight when perfectly dry, nine ounces and a half, its external surface very irregular, of a reddish brown colour, and covered with minute crystals of the ammoniaco-magnesian phosphate. On making a section of it, the internal structure exhibited a radiated appearance, and was of a light brown colour. It consisted of

Phosphate of lime	- -	45.
Ammoniaco-magnesian phosphate	-	28.
Animal matter	- - -	15.
Carbonate of lime	- - -	10.
		98.

In another case the bladder of a horse was found to be nearly full of sand, the composition of which was as follows:

Phosphate of lime	- -	60.
Carbonate of lime	- -	40.
		100.

2. The Ox.

A number of small calculi from the size of a pea downwards, are not unfrequently found in the bladder of the ox. Those in the Hunterian Collection are of a pale brown colour, and of the size just mentioned; some of them have the mulberry appearance.

They consist of carbonate of lime and animal matter, which last substance retains the form of the calculus, after it has been acted upon by diluted acids.

3. THE SHEEP.

A calculus from the kidney of a sheep was composed of

Phosphate of lime	- -	72.
Carbonate of lime	- -	20.
Animal matter	- -	8.
		100.

4. THE RHINOCEROS.

The urine of this animal is exceedingly turbid at the time it is voided, and when allowed to remain at rest, deposites a very large proportion of sediment, which consists of carbonate of lime, with small portions of phosphate of lime and animal matter.

5. THE DOG.

A large calculus from the bladder of a dog twenty years old, weighing sixteen ounces, was extremely hard, and of a gray colour; when cut through, it exhibited a nucleus about the size of a hazel nut, partly made up of concentric layers of phosphate of lime, and partly of crystals of the ammoniaco-magnesian phosphate. The part of the stone surrounding the nucleus consisted of

Phosphate of lime	- - -	64.
Ammoniaco-magnesian phosphate	-	30.
Animal matter	- - -	6.
		100.

Sand taken from a dog's bladder was of a gray colour, and contained

Carbonate of lime - -	20.
Phosphate of lime -	80.
	100.

6. THE HOG.

A calculus from the bladder of this animal weighed nineteen drachms; it was of a pale gray colour inclining to white, and so hard that it was with difficulty cut through. Its internal structure was uniform, and there was no appearance of a nucleus. It was composed of

Carbonate of lime -	90.
Animal matter - -	10.
	100.

7. THE RABBIT.

A calculus from the rabbit's bladder weighing four drachms, was of a dark gray colour, and appeared as if composed of several smaller calculi. It consisted of

Phosphate of lime -	39.
Carbonate of lime - -	42.
Animal matter -	19.
	100.

SECTION VI.

General Inferences.

It appears from the preceding observations, that calculi formed in the kidnies, and immediately voided, are almost always composed of uric acid; and that the phosphates are very frequent ingredients in calculi of the bladder, more especially in those, which, from their situation, have been exposed to a continual current of urine: they also uniformly are deposited upon extraneous substances introduced into the bladder, but appear never to form small kidney calculi.

In what is commonly called a fit of the gravel, a small uric calculus is formed in the kidney, and passes along the ureter into the bladder.

It is found from observation, that for some time after a stone has passed from the kidney, the urine is generally unusually loaded with uric acid, and deposites that substance upon the nucleus now in the bladder. When this period, which is longer or shorter in different individuals, has elapsed, the subsequent addition to the calculus consists principally of the phosphates.

Where the disposition therefore to form uric acid in the kidnies is very great and permanent, the calculus found in the bladder is principally composed of uric acid; but where this disposition is weak and of short duration, the nucleus only is uric acid, and the bulk of the stone is composed of the phosphates.

Where the increased secretion of uric acid returns at intervals, the calculus is composed of alternate layers of uric acid and the phosphates.

Other small calculi being formed in the kidney, make their way into the bladder, and afford fresh nuclei; so that several calculi are sometimes found in the same bladder, and their composition is usually nearly the same.

In other cases it happens, that a constant increased secretion of uric acid is going on from the kidnies, only in small quantity, which will be more uniformly mixed with the phosphates deposited in the bladder, and where the uric acid predominates, the species of calculus denominated improperly, *urate of ammonia*, will be produced.

We are entirely ignorant of the cause of the formation of the oxalate of lime, or mulberry calculus. I have frequently looked for oxalate of lime in the urine of calculous patients, but have never been able to detect it, and as it does not exist in healthy urine, it must be regarded as a morbid secretion. Its mode of formation seems to resemble that of uric acid, since small kidney calculi, composed of oxalate of lime, have in a few instances been voided; and in these cases, as far as my own enquiries go, the persons have been much less liable to a return of the complaint, than where uric calculi have been voided.

In some rare instances we meet with calculi of the bladder which are destitute of uric acid, and of oxalate of lime, the nucleus being composed of a little loosely agglutinated ammoniaco-magnesian phosphate, and the whole calculus consisting of that substance, with variable portions of phosphate of lime: in two cases I have met with calculi of this kind, composed of the triple phosphate only: they seem to be entirely formed in the bladder.

Having taken this short view of the formation of calculi, I shall now enquire into the action of solvents, employed either with a view of effecting their solution, or of preventing their formation and increase.

Solvents are of two kinds.

1. Alkaline. 2. Acid.

In the exhibition of these, the practitioner is usually guided by the chemical composition of the calculous matter voided by urine.

The different kinds of gravel voided by persons labouring under calculous complaints, may be classed in two divisions.

1. *Uric acid*, either in a pure state, or with a very small proportion of the phosphates.

2. *The phosphates*, either pure, or with a small proportion of uric acid.

The first species, which generally appears in the form of minute crystalline grains, of a reddish brown colour, or of an impalpable brown powder, is either entirely soluble in pure alkaline solutions, not emitting an ammoniacal odour, in which case it consists of pure uric acid: or it does emit an ammoniacal odour, and is not entirely soluble, in which case it contains the triple phosphate of ammonia and magnesia.

When this substance is observed in the urine, the alkalies are recommended. They are exhibited either in a pure state, or as carbonates, and in each instance the uric sediment generally diminishes rapidly, and during the continued use of alkaline medicines, occasionally disappears altogether.

It however frequently happens that the matter voided, is not diminished in quantity by the use of alkalies, but that its form and composition are altered, and that it assumes the appearance of a gray powder, and is composed of uric acid with variable portions of the ammoniaco-magnesian phosphate.

From these facts therefore, it cannot be doubted that the internal exhibition of alkalies, often prevents the formation of uric acid, and hence must likewise prevent the increase of a calculus in the bladder, as far at least as uric acid is concerned; but it has also been supposed that the alkalies are capable of acting upon the stone itself, and even of effecting its complete solution. It is true that if we immerse a calculus, composed of uric acid, in a dilute solution of caustic alkali, that it will be slowly acted upon, and after some time entirely dissolved.

If however we attend to what would take place in the body, we shall find the circumstances very different.

That alkaline carbonates and sub-carbonates exert no action upon uric acid I consider to be completely established, both by the experiments of several eminent chemists, and those I have myself made upon the subject; and as there is at all times a quantity of uncombined acid in the urine, it follows that although the alcali may arrive at the kidnies in its pure state, it will there unite with the uncombined acid, and be rendered incapable of exerting any action upon the calculus in the bladder. Besides phosphoric acid, the urine always contains a quantity of uncombined carbonic acid; this is proved by placing a quantity of recently voided urine under the receiver of an air pump; during the exhaustion, a large quantity of carbonic acid gas makes its escape: and when urine is distilled at very low temperatures, carbonic acid gas is given off: and also, when lime water is poured into urine, a precipitate appears, consisting of phosphate and carbonate of lime.

Lime water, on account of the insoluble compounds which lime forms with carbonic, and phosphoric acids, is even more objectionable as a solvent, than the alkalies.

It may however be said, that if these means prevent the increase of a calculus, material relief is afforded to the patient. How far the exhibition of alkaline remedies can be recommended upon these grounds, will appear, when the circumstances which attend the formation of the second species of calculous sediment or deposition in the urine, are considered.

The ammoniaco-magnesian phosphate appears under two forms: it is either voided in a solid state, or in solution. In the former case it bears a good deal of resemblance to a white sand, and is frequently mixed with variable proportions of phosphate of lime. In the latter it makes its appearance after the urine has remained undisturbed for some hours in an open vessel, generally in the form of a fine pellicle, or of crystalline laminæ, which when collected and dried bear some resemblance to boracic acid.

Its putting on this form is accounted for, from its being held in solution in the first instance by carbonic acid, and as this flies off, the triple salt makes its appearance. If a portion of the urine be preserved in a phial closely stopped, the carbonic acid cannot escape, and consequently no phosphate is observed to separate. There is also a quantity of phosphoric acid present, which keeps another portion of the ammoniaco-magnesian phosphate, and also some lime (in the state of super phosphate of lime) in solution.

It is therefore obvious, that whenever the urine is deprived of a portion of the acid which is natural to it, the deposition of the triple phosphate, and phosphate of lime, more readily takes place: this is effected by the exhibition of the alkalies.

It may therefore be asserted, that although alkaline medicines often tend to diminish the quantity of uric acid, and thus to prevent the addition of that substance in its pure state, to a calculus in the bladder; they favour the deposition of the phosphates.

It cannot be doubted that the alkalies reach the bladder, since in cases where large doses of sub-carbonate of potash have been exhibited, I have seen evident traces of it in the urine.

Where the phosphates only are voided, it has been proposed to dissolve the calculus by the exhibition of acids, and more especially the muriatic acid.

During the use of the muriatic acid, the phosphates are either diminished or disappear altogether; and even sometimes the urine acquires an additional acidity: a solution of that part of the calculus which consists of the phosphates might therefore be expected; but even then the nucleus of uric acid would remain, and thus a great deal of time would be lost without any permanent advantage.

I have also occasionally remarked, that during the use of acids, the uric acid re-appears, and even seems to be augmented in quantity.

Attempts have been made at different times to effect the solution of calculi, by the injection of solvents into the bladder. This subject has been more lately revived by FOURCROY and VAUQUELIN, who, in their paper on the composition of calculi, lay down rules for its practice. Independent, however, of the impossibility of ascertaining the composition of the calculus with sufficient accuracy, it is obvious, that were the composition of the surface of the calculus known, the frequent introduction of an instrument into the bladder, and the long continuance of the process which would be necessary, even where the calculi are small, are insurmountable objections; and whenever this mode of treatment has been adopted, it has speedily been relinquished, as it always aggravates the sufferings of the patient.

It has been shewn that in the majority of cases, the nuclei of calculi originate in the kidnies, and that of these nuclei by far the greater number consist of uric acid; the good effects therefore so frequently observed during the use of an alkali, arise, not from any actual solution of calculous matter, but from the power which it possesses of diminishing the secretion of uric acid, and thus preventing the enlargement of the calculus, so that, while of a very small form, it may be voided by the urethra.

XVI. *Some Observations on Mr.* Brande's *Paper on Calculi.*

By Everard Home, *Esq. F. R. S.*

Read May 19, 1808.

That calculi in the human bladder are not dissolved by the internal use of alkaline medicines, is an opinion which I have long entertained, but the grounds of failure so clearly pointed out by Mr. W. Brande, were not known to me: I only knew from experience, that, to whatever extent the medicines are given, no such effect takes place. The circumstance of the exterior laminæ of calculi extracted from patients, who had persevered in a course of alkaline preparations, having been found softer than the parts towards the centre, has always been considered as a proof of the action of the medicines upon the calculus, and led to the belief, that where the stone was small, it might be wholly dissolved. This, however, Mr. W. Brande has now proved to be a deception, and that the soft part is not a portion of the original calculus, but a newly formed substance, in which the uric acid is not deposited in crystals, but mechanically mixed with the phosphates, and the animal mucus in the urine.

Having met with cases, which confirm Mr. W. Brande's observations, it will be satisfactory to state them, as they may assist in doing away many erroneous notions generally entertained on this subject.

The opinion, that calculi in the human bladder have been entirely dissolved, has received its principal support from instances having occurred, and those by no means few in number, where the symptoms went entirely away while the patients were using alkaline medicines, and never afterwards returned. This evidence appears to be very strong, but it will be found from the following cases that it is not so in reality. Since the fallacy has been detected in all the instances in which an opportunity was afforded of examining the bladder after death. Two of these I shall particularly notice, because they were published during the patients' life time in proof of the stone having been dissolved.

Both patients were great sufferers from the symptoms of stone for many years; but when they arrived at the age of sixty-eight, or thereabout, the symptoms entirely left them. The one had been taking the saline draught in a state of effervescence, under the direction of the late Dr. HULME: the cure was attributed to this medicine, and the case was published in proof of its efficacy. When the patient died I examined the bladder, and found twenty calculi; the largest of the size of a hazel nut, the others smaller. It appeared that the going off of the symptoms had arisen from the posterior lobe of the prostate gland having become enlarged (a change which it frequently undergoes about that period of life,) and having formed a barrier between the calculi and the orifice of the bladder, so that they no longer irritated that part either in the act of making water, or in the different movements of the body, but lay in the lower posterior part of the bladder without producing any disturbance. Their number prevented the pressure from being great upon any one part of the intestine immediately behind the bladder, and their motion on one another rendered their external surface smooth, and probably prevented their rapid increase. The other patient was under a course of PERRY's lixivium; and when the symptoms went away, he published the case in proof of the efficacy of that medicine in dissolving the stone. I examined the bladder after death, and found fourteen calculi; the largest of the size of a nutmeg, the others smaller. There was the same enlargement of the posterior lobe of the prostate gland, and the calculi were exactly under the same circumstances as in the former case.

In several cases, in which I have examined the body after death, calculi have been found inclosed in cysts, formed between the fasciculi of the muscular coat of the bladder, so as to be entirely excluded from the general cavity, and therefore had not produced any of the common symptoms of stone. I have seen in the same bladder, two, three, and even four such cysts, each containing a calculus of the size of a walnut.

It is a circumstance deserving notice, that in the case, which gave celebrity to Mrs. STEVENS's medicine, and procured her a remuneration from Parliament, the bladder was not examined after death.

That calculi in the bladder do sometimes increase, while the patient is using alkaline medicines, is fully proved by the following examples, which also shew that the uric acid and phosphates are formed in different proportions, according to the peculiarities of the constitution.

A gentleman who suffered from symptoms of stone was sounded and a stone was found in his bladder. I put him on a course of alkaline medicines, and he voided a small compact calculus, composed of uric acid, and evidently formed in the kidney. He was desired to persist in the use of the medicines, which he did at intervals for four or five years, suffering occasionally in a slight degree, but he did not pass any more calculi. He died at the age of seventy-five. On examining the bladder, its whole cavity, (the capacity of which was equal to a pint measure) was completely filled with soft, light, spongy calculi, three hundred and fifty in number, and of different sizes, from that of a walnut to a small pea. They were composed of a mixture of uric acid in powder, the phosphates, and animal mucus; and differed so much from the calculus voided soon after the patient began the use of alkalies, that they appear to have been formed after that period in the manner mentioned by Mr. W. Brande.

A gentleman, who was found to have a stone in his bladder, was persuaded that it was so small that it might be dissolved, and with this view he took the fossil alkali, both in its caustic and mild state, for about three months; but at the end of that period the symptoms were encreased, and he submitted to have it extracted by an operation. On examinining the calculus after it was extracted, the external part, for the thickness of $\frac{1}{10}$ of an inch, was entirely composed of triple phosphate, in a state of perfect spiculated crystals, so as to present a very rough irritating surface to the internal membrane of the bladder, while the inner parts of the calculus were made up of a mixture of uric acid and phosphates, so that the alkali had prevented the formation of uric acid, but the phosphates were deposited more rapidly than before.

A gentleman, in whose urine the uric acid appears in a solid form, immediately after it is voided, has the same appearance in the urine, even when nine drachms of soda dissolved in water, impregnated with carbonic acid, are taken in twenty-four hours; so that in this instance the alkali does not even counteract the formation of uric acid.

XVII. *On the Changes produced in Atmospheric Air, and Oxygen Gas, by Respiration.* By W. Allen, *Esq.* F. R. S. *and* W. H. Pepys, *Esq. F. R. S.*

Read June 16th, 1808.

The process of respiration, or breathing, is so intimately connected with our existence in life, that from its first moments, to the final close, sleeping and waking, this necessary action is constantly maintained: nor can it be suspended even for a few minutes without considerable pain and the utmost danger. This important process has of course excited the curiosity both of ancient and modern philosophers; among the latter we find the distinguished names of Mayow, Priestley, Goodwin, Menzies, Spallanzani, Scheele, Lavoisier and Davy, whose successive labours have thrown great light upon this difficult subject, and prepared the way for farther investigation; but it is impossible to take a review of what has already been done, without perceiving that some important points were by no means satisfactorily settled; an accurate method of separating the different gasses, and ascertaining their exact proportion in any given mixture, was still a desideratum when many of the experiments were made, and it is only of late years that Eudiometry has attained its present perfection: the quantity of residual gas in the lungs after a forced expiration was a matter in dispute among former experimenters, some making it one hundred and nine cubic inches, and others only forty;

and yet it is of the utmost consequence in all calculations upon the effects produced, especially upon small portions of gas, that the state of the lungs should be accurately determined; this constitutes the great difficulty in the investigations. We therefore commenced our labours by constructing an apparatus, in which we are able to respire from three to four thousand cubic inches of gas, conceiving, that in this quantity, the error arising from the residual gas in the lungs must be so much obviated as to permit the most satisfactory results.

The apparatus consists of three gasometers, two of which are filled with mercury, and one with distilled water.

The water gasometer which belongs to the Royal Institution, is capable of holding four thousand two hundred cubic inches of gas, and each of the mercurial ones three hundred cubic inches: the apparatus was so arranged that the inspirations were all made from the water gasometer, and the expirations into the mercurial gasometers alternately. Each of the gasometers is furnished with a graduated scale, and they are all made to range with each other, so that the quantity of gas inspired and expired could be immediately and exactly ascertained: to each of the mercurial gasometers a glass tube is fixed, and made to enter a mercurial bath, from which portions of the expired air could at any time be taken for examination.

By the kindness of our friend Silvanus Bevan, we are enabled to give an accurate drawing of the apparatus.

Description.

Fig. 1. The communication with the water gasometer.

2. A cock so constructed that it might be made to communicate with either of the mercurial gasometers, while at the same time all connexion with the other was cut off.

A. The mouth piece.

Fig. 3. to 10. Brass cocks.

G. 1. and G. 2. Mercurial gasometers.

S. S. Scales graduated to cubic inches.

M. Mercurial bath.

The large reservoir or water gasometer is not shewn in this drawing, it having been so frequently described in chemical works.

Manner of conducting the Experiment.

Our first care was, to be certain that all the parts of our apparatus were perfectly air-tight, and this, from the nature of it, was very easily ascertained; we agreed that the breathing should always be performed by one of us, and the registering, &c. by the other, as each would by that means acquire a greater degree of dexterity in performing his part, and the results would be more uniform.

The water gasometer being filled with common air to a certain mark upon the scale, and the mercurial ones completely empty, the person to breathe, whom we shall uniformly call the operator, was seated upon a stool, with his mouth even with the tube A., his nose being secured with a steel clip. He made as complete an expiration as possible into the open air, then applying his lips to the tube, and keeping his left hand constantly on the cock, fig. 1, and his right hand on the cock, fig. 2, he opened the communication with the water gasometer, and made an inspiration; then immediately closing it, he opened with his right hand the cock at 2; and that at 4 being also opened, he expired into the mercurial gasometer G. 1.; then closing 2, which cut off all communication with the mercurial gasometer, he opened 1, in order to make a fresh inspiration; then closing it, he again opened 2, and expired into the mercurial gasometer; and proceeding in this way, always taking care to shut one cock before the other was opened, the air was made to pass from the water gasometer, through the lungs of the operator into the mercurial gasometer, and this with great ease, as the diameters of the tubes were purposely made large. The scale of the mercurial gasometer was carefully noticed, and when nearly full, the cock 4 underneath was shut off: then, by a signal from the operator, his colleague opened 3, and the expirations were received in G. 2. While this was filling, the number of cubic inches in G. 1 was registered, a portion saved in the mercurial bath, and the rest quickly expelled. This operation was repeated until the contents of about twelve or thirteen mercurial gasometers were taken off; the operator always concluding with a strong effort to empty his lungs as completely as possible. The quantity inspired from the water gasometer was then compared with the quantities expired into the mercurial gasometers, and the difference noted. The following are the results of the first ten experiments.

No.	Time.	Cubic inches of common air Inspired.	Cubic Inches of Gas Expired.	Deficiency.
1.	time not noted	3760	3741	19
2.	11 minutes	3900	3869	31
3.	10½ ———	3624	3620	4
4.	10½ ———	3570	3550	20
5.	11 ———	3685	3653	32
6.	11 ———	3380	3355	25
7.	10½ ———	3180	3141	39
8.	10½ ———	3360	3298	62
9.	10½ ———	3290	3267	23
10.	11 ———	3580	3543	37

In this last experiment we ascertained that the expired gas contained 8 per cent. of carbonic acid.

The breathing in these cases was as nearly natural as we conceive it possible to be in any apparatus; the operator was scarcely fatigued, and his pulse not raised more than about one beat in a minute; the respirations however were deeper and fewer than natural, amounting only to about 58 in eleven minutes, whereas from repeated observations at different and distant times he makes 19 in a minute. The smallness of the deficiency surprised us very much, as, from the reports of other experimenters we had been prepared to expect a much greater loss. It might be objected that the air was rarefied by passing through the lungs; but this was almost immediately counteracted by the mass of quicksilver in the gasometers, which amounted at least to one hundred and fifty pounds; and we have repeatedly noticed, that air under these circumstances has suffered no perceptible diminution by standing for a considerable time; in one case, in which air from the lungs was driven into the mercurial gasometers for twenty-seven minutes, the temperature of the quicksilver at the end of the experiment was not raised half a degree of FAHRENHEIT's thermometer. The deficiency, in our opinion, principally arises from the difficulty in bringing the lungs precisely to the same state after, as before the experiment; and it must be recollected that the operator commenced by a forcible expiration into the open air, but finished by a forcible expiration into the mercurial gasometer. Now, although this gasometer was counterpoised by weights in the scale attached to it, yet we can easily conceive that more resistance might be afforded to the complete evacuation in the latter case than in the former, and consequently the lungs might contain a few inches more after the experiment than before it, which might in some measure account for the deficiency.

In the eleventh experiment, portions of gas were taken off from each of the mercurial gasometers as they were filled, and these portions being afterwards mixed were carefully examined.

Eleventh Experiment.

Barom.	Thermom. Faht.	Time.	Cubic Inches of common air Inspired.	Cubic inches Expired.	Deficiency.
30,4	50°	11 min.	3460	3437	23

To prevent repetition, we shall here state that all the trials were made in the same manner, and with the same apparatus, namely, the Eudiometer, described in the Society's Transactions for 1807, in which one cubic inch is divided into one hundred parts; and that in almost every instance we made two, and sometimes three experiments on the same gas, and derived fresh confidence from the remarkable coincidence and uniformity of the results. No precaution was at any time omitted which appeared to us necessary to insure accuracy.

One hundred parts of the expired gas being agitated with lime water in the eudiometer, the lime water became turbid, and 8.5 parts of the gas were absorbed, which were consequently carbonic acid; the remaining 91.5 parts were treated with the green sulphate of iron, saturated with nitrous gas, as recommended by Professor DAVY, and afterwards with the simple solution of the green sulphate, when 12.5 parts were absorbed, which were consequently oxygen, and the remaining 79 azote.

100 parts of the expired gas therefore consisted of

8.5 carbonic acid.
12.5 oxygen.
79. azote.
———
100

The air contained in the water gasometer, previous to the experiment, being examined by the same tests, consisted in 100 parts of

21 oxygen.
79 azote.
———
100

In trying common atmospheric air with lime water, we could never find any quantity of carbonic acid perceptible in the eudiometer of 100 parts.

Calculation for Carbonic Acid.

$$100 : 8.5 :: 3437 : 292.145.$$

So that 292.14 cubic inches of carbonic acid gas were given off in eleven minutes, or 26.55 cubic inches per minute, which is almost exactly the estimate of Professor DAVY.

In this experiment the operator inspired 3460 cubic inches in eleven minutes, and felt himself in a natural state when he left off. Then, as he makes usually under common circumstances nineteen respirations in a minute,

$$11 \times 19 = 209 \qquad \frac{3460}{209} = 16.5$$

it follows, that he takes in 16½ cubic inches at every easy inspiration.

As all the experiments had been hitherto made upon the lungs of one person, we concluded that the next should be performed upon our assistant.

Twelfth Experiment.

Barom.	Therm. Faht.	Time.	Cub. inches of common air Inspired.	Cubic Inches Expired.	Difference.
30.3	56°	5½ min.	3300	3311	11 increase

Here, as usual, the lungs were exhausted both before, and at the close of the experiment.

The excess of eleven cubic inches, in this case, no doubt arose from the person not having been in the habit of exhausting his lungs, so that they contained more when he began than when he left off; his lungs appeared to be of greater capacity than those of the usual operator.

Portions of gas were saved from each of the mercurial gasometers as they were filled, which being mixed together, for the average gave the following results:

100 parts of the mixture contained

8.5 carbonic acid
12.5 oxygen.
79 azote.
———
100

Calculation for Carbonic Acid.

100 : 8.5 : : 3311 : 281.43.

Consequently 281,43 cubic inches of carbonic acid gas were given off in 5½ minutes.

In this experiment we meet with a remarkable fact, viz. that as much carbonic acid gas was given off in 5½ minutes, as in the former experiment in eleven minutes; so that it appears, whenever atmospheric air is taken into the lungs, it returns charged with about 8 per cent. carbonic acid. The faster respiration is performed, the more carbonic acid is given off, and consequently the more oxygen consumed: in this instance it was given off at the rate of fifty-one cubic inches per minute.

Thirteenth Experiment.

We now proceeded to carry on the respiration of common air for a much longer period than usual, and of course on a much larger quantity. The experiment was made by the same operator who had performed all the others, except the 12th. Eleven mercurial gasometers having been filled, taken off, and registered, the operator continued to breathe in the 12th until a mark was made by his colleague upon the scale of the water gasometer, and it was again filled with common air to the usual division on the scale. This occupied but a very short space of time. The operator, without taking his lips from the tube, then filled twelve more of the mercurial gasometers, which were registered as before, and he continued to breathe in the 12th, until the water gasometer was again replenished; eleven more were then filled, and portions saved from each: the experiment was completed by a forcible expiration of 166 cubic inches into the 12th; and this last portion being left for an hour and a half was not perceptibly diminished in volume.

Barom.	Therm. Faht.	Time.	Cub. inches of common air inspired.	Cub. inches expired.	Deficiency.
29,85	68°	24′.37″	9890	9872	18

The breathing was so nearly natural that the operator was scarcely fatigued, and thought that he could have gone on for a much longer time.

The smallness of the deficiency, notwithstanding the experiments occupied 24½ minutes, is a striking circumstance, and leads us to suspect still more strongly, that the deficiency principally arises from the impossibility of always bringing the lungs to the same state after forcible expiration.

100 parts of the mixture of expired gas gave

8 carbonic acid,
13 oxygen,
79 azote.
———
100

Calculation for Carbonic Acid.

100 : 8 : : 9872 : 789,76.

So that 789,76 cubic inches of carbonic acid gas were given off in 24½ minutes, which gives thirty-two cubic inches per minute. But here it must be noticed that the respiration was more rapid than in the 11th experiment, and a larger quantity of carbonic acid given off in the same time. This agrees with the 12th experiment.

We are very much inclined to think that, in ordinary respiration, a great part of the air is returned unaltered, viz. that contained in the fauces, in the trachea, and probably a portion of that in the larger branches of the bronchia. If this circumstance be not adverted to in experiments upon small quantities of air, the results can never be correct. There is even a considerable difference in the quality of the first and last portions of a single inspiration. In some experiments made with a view to this subject, a small quantity of the first portions, given off in a common and natural expiration, was received in a vessel over mercury; on examination, it only contained 3.5 per cent. carbonic acid; in other experiments the first portions contain from three to five per cent.; while the general average appears by the 11th, 12th, and 13th experiments, to be about eight.

The operator, after rather more than a natural inspiration, expired 204 cubic inches into the mercurial gasometer, making his utmost efforts to press as much as possible out of the lungs, this contained 9.5 per cent. of carbonic acid. Here we are to recollect, that these 204 cubic inches contained the first, as well as the last portions; the first portions have been proved to contain only from three to five per cent.; consequently the last portions must contain more than appears by the average; that is, more than 9.5 per cent.

It now appeared to us of consequence to ascertain exactly what happened to a given volume of atmospheric air, when it was inspired and expired as often as possible.

*Fourteenth Experiment.**

Three hundred cubic inches of atmospheric air were admitted into the mercurial gasometer G. 1.; the other, G. 2, was empty. The nose being properly secured, and the mouth applied to the tube A, as usual, air was drawn from G. 1, and by half turning the cock, 2, was expired into G. 2. This was repeated until the contents of G. 1 had been made to pass through the lungs, and transmitted to G. 2. The air was then inspired from G. 2, and expired into G. 1, until G. 2 was nearly empty. This was repeated about eight or ten times during three minutes, until respiration became extremely laborious, and the operator desisted.

The whole 300 cubic inches must have passed eight or ten times through the lungs; and we confidently expected, that on examining the air we should have found an unusual proportion of carbonic acid.

But 100 parts gave only 9.5 carbonic acid
5.5 oxygen
85. azote.
100

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* In this experiment there was obviously no occasion to make allowance for the air contained in the tubes and sockets. We find its volume to be eighteen cubic inches.

Here was an increase of six parts in 100 of something which the tests for oxygen would not take up, and also a loss of six per cent. oxygen. This seemed to convince us, that under certain circumstances, as during some peculiar alteration in the vital functions, gaseous oxide of carbon, carburetted hydrogen, or some other gas not absorbable by lime water or the tests for oxygen, might be given off from the lungs, and we accordingly determined to repeat CRUIKSHANK's experiments with hyperoxygenised muriatic acid gas.

We procured the gas from hyperoxygenised muriate of potash by means of muriatic acid, and mixing it with a known portion of gaseous oxide of carbon in a flint stopper bottle, the mouth of which was immersed in mercury for twenty-four hours, the gaseous oxide of carbon was converted into carbonic acid gas, as was proved by its effects upon lime water, which, when both the gasses are pure, absorbs them entirely after they have remained together for twenty-four hours; it was plain, therefore, that we had the means of detecting gaseous oxide of carbon, and doubtless carburretted hydrogen, if any should be contained in the expired gas. From a conviction of the importance of these experiments we were determined to take nothing upon trust.

Fifteenth Experiment.

We repeated the 14th experiment with a little variation. In this case we employed only one of the mercurial gasometers, into which exactly 300 cubic inches of atmospheric air were admitted. The operator having made an easy expiration, applied his mouth to the cock at the top of the bell glass, and the time being noted, began to breathe; in less than a minute he found himself obliged to take deeper and deeper inspirations; and at last the efforts of the lungs to take in air became so strong and sudden, that the glass was in some danger of being broken against the side of the gasometer. A great sense of oppression and suffocation was now felt in the chest, vision became indistinct, and after the second minute his whole attention seemed to be withdrawn from surrounding objects and fixed upon the experiment. He now experienced that buzz in the ears which is noticed in breathing nitrous oxide, and after the third minute had only sufficient recollection to close the cock after an expiration. This secured the result of the experiment; but he became so perfectly insensible that, on recovering, he was much surprised at finding his friend and the assistant on the table in the act of supporting him. It was noticed that he made thirty-five inspirations during the experiment. We now examined the air which had been so treated.

100 parts contained	10	carbonic acid,
	4	oxygen,
	86	azote,
	100	

In this experiment it is remarkable, that the air which had been so often through the lungs, should only have furnished 10 per cent. of carbonic acid, while the air which passes them but once contains from 8 to 8.5.

Here the oxygen had lost 7 from 21, and the azote had gained 7 upon 79.

We knew by previous experiment,* that every cubic inch of carbonic acid gas required exactly a cubic inch of oxygen for its formation; the ten parts of carbonic acid may therefore be reckoned as oxygen, which would make the constitution of the gas after the experiment $\begin{cases}\text{14 oxygen,}\\ \text{86 azote,}\end{cases}$ whereas before the experiment it was $\begin{cases}\text{21 oxygen,}\\ \text{79 azote.}\end{cases}$

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* See the experiments on carbonic acid in the Society's Transactions.

Now we did not suppose the residuum of 86 to be all azote, though 79 might be; therefore seven parts appeared to have been added by this unnatural mode of respiring, and we conjectured the addition might be gaseous oxide of carbon.

To ascertain this, we put 40 parts into a flint stopper bottle, and nearly filled it with about 100 parts hyperoxygenised muriatic acid, procured as before, and recently prepared; the stopper being put in, over distilled water, we plunged it in quicksilver, and filled a second bottle in the same way, as a comparative experiment.

We next procured some pure azote, by absorbing the oxygen from a portion of atmospheric air by the saturated green sulphate and simple green sulphate as usual; 40 parts of this azote were mixed with the same proportion of the acid gas as in the other experiment, and the whole suffered to stand for forty-eight hours; at the end of this time the azote was examined, by washing it first in distilled water, and afterwards in the eudiometer with the tests for oxygen; and there were still exactly 40 parts left; proving that the hyperoxygenised muriatic acid gas has no action upon azote.

We then examined the bottles containing the residuum from the air that had been so often respired, and found that it had not experienced the slightest change; it was therefore plainly azote; and on reflection, it occurred to us, that if a certain proportion of oxygen had been absorbed or lost in any way, while the azote remained unaltered, there must be an increased proportion of the latter.

Now we knew exactly both the bulk and the constitution of the air before the experiment; but it was impossible to know the bulk or volume after the experiment otherwise than by calculation.

The 300 cubic inches of atmospheric air before the experiment contained 21 oxygen, 79 azote in 100 parts, making the total quantity of oxygen 63 cubic inches,

$$\begin{array}{lr} & 63 \\ \text{azote} & 237 \\ \hline & 300 \end{array}$$

Now if the lungs be capable of fixing permanently any azote from the atmosphere, it appears by our experiments that the quantity must be very minute, seeing that in the 11th, 12th, and 13th experiment, it did not disturb the proportion of azote, as shewn by the eudiometer; we shall therefore in the present instance assume the volume of azote after the experiment at 237 cubic inches, as before.

But after the experiment, every 100 parts consisted of 86 parts azote, and 14 oxygen, either in the form of carbonic acid, or free.

$$86 : 14 :: 237 : 38.58.$$

Therefore the total quantity of oxygen left after the experiment would have been 38.58 cubic inches.

Then 237 azote + 38.58 oxygen = 275.58; the quantity of gas after respiration would therefore have been 275.58 cubic inches.

300 — 275.58 = the loss of oxygen, or 24.42 cubic inches.

It appears, therefore, that 24.42 cubic inches of oxygen had been absorbed by the system under the circumstances of this experiment.

Reviewing the 14th experiment, it appears that the gas after respiration contained 85 per cent. azote, and 15 per cent. oxygen, either in the state of carbonic acid, or free.

State of the Air before the Experiment.

300 = 237 azote + 63 oxygen.

After the Experiment.

85 : 15 :: 237 : 41.82.

The total quantity of oxygen after the experiment appears to be 41.82 cubic inches.

Then 237 azote + 41.82 oxygen = 278.82.

The total volume after the experiment appears to be 278.82 cubic inches.

300 — 278.82 = 21.18.

The loss of oxygen in this case was 21.18 cubic inches.

We are disposed to consider the 11th as a standard experiment relative to carbonic acid gas, because the quantity of air respired in a given time is pretty near the average of the first ten experiments; and because it very nearly agrees with the statement of Professor Davy. In this experiment 292 cubic inches of carbonic acid gas were given off in eleven minutes; the barometer was 30.4, the thermometer 50°, the volume being calculated at the mean, viz. barometer 30, thermometer 60°, will be 302 cubic inches given off in eleven minutes, or 39534 cubic inches in twenty-four hours, supposing the production to be uniform during all that period;

and as 100 cubic inches of carbonic acid gas weigh 47.26 grains,

100 : 47.26 : : 39534 : 18683.76;

the weight of the carbonic acid gas amounts to 18683.76 grains; and estimating the carbon in it at 28 parts in 100, according to Lavoisier, or 28.60, as calculated in the experiments on diamond, recorded in the Society's Transactions.

100 : 28.60 : : 18683.76 : 5363.55 grains;

it will follow that 5363.55 grs. or above 11 oz. troy of solid carbon, are emitted by the lungs in the course of twenty-four hours; and that 39534 cubic inches of oxygen gas are consumed in the same time. But when we consider that in respiration perfectly natural, a much smaller quantity of air can come in contact with those parts of the lungs calculated to act upon it, the proportion of carbonic acid gas given off in natural respiration, ought probably to stand considerably lower than in the above estimate; but at all events it will be very considerable.

Sixteenth Experiment.

Having made so many experiments upon atmospheric air, we now proceeded to ascertain the effects produced upon oxygen gas by the process of respiration. The water gasometer was filled with oxygen gas made from the hyperoxygenised muriate of potash by heat, care having been taken to clear all the tubes, &c. as much as possible of common air, by forcing a quantity of oxygen gas through them.

One hundred parts from the water gasometer being treated with the usual tests in the Eudiometer, a residuum of only 2.5 was left; so that 97.5 per cent. were pure oxygen, and the rest azote.

The register of the water apparatus being noticed, and the operator having prepared himself as usual by a forced expiration, began to respire; his pulse was 72; and at the end of 9 minutes and twenty seconds, the experiment was concluded by a forced expiration, when the pulse was raised to 88.

Barom.	Therm. Faht.	Time.	Cubic Inches of oxygen gas inspired.	Cub. Inches expired.	Deficiency.
29.5	53°	9′:20″	3260	3193	67

The operator felt a general glow over the body to the very extremities, with a gentle perspiration; this however went off in a few minutes, and no remarkable deviation from the ordinary state was experienced.

A portion having been saved, as usual, from each of the mercurial gasometers, for an average,

100 parts contained
11 carbonic acid,
83 oxygen,
6 azote.

100

The examination repeated, gave the same results.

Calculation for Carbonic Acid.

100 : 11 : : 3193 : 351.23,

consequently, 351.23 cubic inches of carbonic acid gas were formed in 9′.20″, or 37.64 cubic inches in a minute.

Here it is plain that a greater quantity of carbonic acid was formed from oxygen than from common air, in the same time, and hence we infer, that one use of azote is to regulate the quantity of oxygen, which shall be taken up in the act of respiration.

The gas inspired was 3260 cubic inches, and of this 2.5 per cent. was azote.

100 : 2.5 :: 3260 : 81.50.

The total quantity of azote in the gas inspired, was therefore 81.50 cubic inches.

The quantity of gas expired was 3193 cubic inches, and of this every 100 parts contained six of azote.

100 : 6 :: 3193 : 191.58.

The total quantity of azote in the gas expired, was therefore 191.58 cubic inches; but the total quantity of azote before respiration was only 81.50.

191.58 — 81.50 = 110.08;

therefore 110.08 cubic inches were added by the process of respiration, beside what little remained in the lungs after the experiment.

Calculation for Oxygen.

The 3260 cubic inches of gas inspired contained 81.50 azote.

3260 — 81.50 = 3178.50,

and consequently the pure oxygen was 3178,50 cubic inches. The 3193 cubic inches of gas expired, contained 83 per cent. of free oxygen, and 11 per cent. in carbonic acid gas, making together 94.

100 : 94 :: 3193 : 3001.42.

The oxygen gas, found after the experiment, was therefore 3001.42 cubic inches, and deducting this from the oxygen before the experiment,

3178.50 — 3001.42 = 177,08.

It appears, at first sight, that 177.08 cubic inches of oxygen were missing, but great part of this may be accounted for, by adverting to the state of the lungs after the experiment.

The addition of 110.08 cubic inches of azote, we consider as arising from that portion still retained in the lungs, notwithstanding the forced expiration at the beginning of the experiment, and considering that in the 14th and 15th experiment, where the same air was repeatedly breathed, the proportion of azote was in the one case 85, and in the other 86 per cent. It seems fair to presume, that the residual air contained in the lungs after a forced expiration may amount in 100 parts, to not more than 16 oxygen and 84 azote: any one who reflects upon the structure of the lungs, and the minute ramifications of the extremities of the bronchial vessels; and when he also considers that those parts of the lungs with which the air comes in contact, if spread out, would present a surface equal to that of the superficies of the whole body; and lastly, that this viscus is so exceedingly spongy and porous, that when once inflated, it is ever after impossible by ordinary mechanical means to expel the air completely, he will easily perceive, not only that a large portion of air must remain for a considerable time in contact with the internal surface of the lungs, where it is liable to lose a portion of its oxygen, but also that the residual quantity of air in the lungs, after the most violent attempts at expiration, may be very considerable. It is to this circumstance that we attribute the excess of azote in the experiments upon oxygen, and pretty deep inspirations of this gas having been made during 9′.20″, the azote must have been in great measure displaced. Admitting then that the air contained in the lungs, before the experiment, consisted of 16 oxygen, 84 azote, and at the conclusion of the experiment of 94 oxygen, 6 azote, then we have

$\frac{84x}{100}$ azote at the begining,

$\frac{6x}{100}$ azote at the end.

$$\frac{6x}{100} + 110 = \frac{84x}{100},$$

$$110 = \frac{84x}{100} - \frac{6x}{100} \text{ or, } 84x - .06x = .78x.$$

$$x = \frac{110}{,78} \text{ or } 141 \text{ cubic inches ;}$$

Therefore upon this calulation it appears that 141 cubic inches of gas remained in the lungs after a forcible attempt at expiration; then the state of the lungs before the experiment must have been

118.44 azote,
22.56 oxygen.

141

And after the experiment,

132.54 oxygen,
8.46 azote.

141

Calculation on total Quantities.

Azote before the experiment 81.50 cubic inches,
—— contained in the lungs 118,44

199.94

Azote after the experiment,	
—— found by the tests	191.58
—— contained in the lungs,	8.46
	200.04
Oxygen before the experiment,	3178.50
—— contained in the lungs,	22.56
	3201.06
Oxygen after the experiment,	
Found by the tests,	3001.42
Contained in the lungs,	132.54
	3133.96
Total of oxygen before the experiment,	3201.06
Total of oxygen after the experiment,	3133.96
Difference	. . 67.10

The deficiency noticed in the experiment was 67, supposing that the lungs were brought to the same state after as before the experiment; but granting that this was not the case, and that at the close of the experiment the state of the lungs was 141 + 67 = 208, still our approximation will come within four or five cubic inches, for the azote contained in the sixty-seven missing would be only about four cubic inches. We are aware that the temperature of the lungs being 97, while that of the gas was 53°, the 141 cubic inches would occupy a space equal to 154 cubic inches; but this residual quantity must be greater or less according to the exertion made, and also probably according to the state of the muscular fibre at the time.

Seventeenth Experiment.

The water gasometer was filled to the usual mark upon the scale, with oxygen gas, prepared from about 9 oz. troy, of hyperoxygenised muriate of potash, as in the former experiment; the gas being examined was found to contain as before, 2.5 azote, and 97.5 oxygen, in 100 parts.

The apparatus being found air tight, and all the tubes, &c. cleared of atmospheric air by passing oxygen through them, the operator prepared himself for the experiment; but it must be noticed that he had been rather fatigued during five hours previous to respiring, and had not taken any refreshment; the weather was very warm; his pulse 86; heat under the tongue 98¾; he felt no uncomfortable sensation during the process, but experienced a gentle glow and universal perspiration, breathing all the time with great ease, his pulse after the experiment was 102, and the heat under the tongue 99°.

Barom.	Therm. Faht.	Time.	Cub inches. of oxygen gas inspired.	Cub. inches expired.	Deficiency.
30.3	70°	7'.25''	3420	3362	58.

The quantities of expired gas taken off in each of the mercurial gasometers were as under, in the order in which they were filled.

No.	1. - -	250 cubic inches.
	2. - -	290
	3. - -	272
	4. - -	238
	5. - -	252
	6. - -	300

7.	-	-	241
8.	-	-	296
9.	-	-	256
10.	-	-	256
11.	-	-	286
12.	-	-	257
13.	-	-	168
			3362

The 13th gasometer was the whole of the last single and forcible expiration, portions were saved from each of the gasometers, and we first examined the state of No. 1.

100 parts contained 9 carbonic acid,
25 azote,
66 oxygen,
100

The large quantity of azote in this case, was a clear proof that our conjecture upon the residual gas in the lungs was well founded.

We then examined a mixture of No. 2 and 3.

100 parts contained 10.5 carbonic acid,
10 azote,
79.5 oxygen,
100

here the quantity of azote was diminishing, and the ratio of carbonic acid increasing, so that it appears necessary for the lungs to be cleared of azote, before the increased proportion of carbonic acid can take place.

The 13th or last gasometer was now examined by itself;

100 parts contained 12.5 carbonic acid,
5.5 azote,
82. oxygen.
100

Here the proportion of azote was only 3 per cent. more than what existed previously in the gas, and hence we may conclude, that even seven minutes and a half was not a sufficient time to remove the azote from the extremities of the bronchia.

We lastly made a mixture of all the gasometers, from 2 to 12 inclusive, and found that 100 parts contained

12 carbonic acid,
6.5 azote,
81.5 oxygen,
100

Calculation for Carbonic Acid.

100 : 9 :: 250 : 22.50 Carbonic acid gas in No. 1. 22.50
100 : 12.5 :: 186 : 21 ditto No. 13. 21
From 3362 total expired
250 No. 1.
168 No. 13
Deduct 418
Leaves the mixture 2944 of No 2. to No. 12.
100 : 12 :: 2944 : 353.28 Carb. acid gas in mixture 2. to 12. 353.28
396.78

The total quantity of carbonic acid gas emitted, was therefore 396.78 cubic inches.

Calculation for Azote.

100 : 25 :: 250 : 62.50	Azote in No. 1.		62.50
100 : 5.5 :: 168 : 9.24	—— in No. 13.		9.24
100 : 6.5 :: 2944 : 191.36	—— in mixt. 2. to 12.		191.36
			263.10

The azote expired, beside what might be contained in the lungs, at the close of the experiment, was therefore 263.10 cubic inches. Here it is plain, that the operator, at the beginning of this experiment, had not brought his lungs to the same state as in the preceding; or that in consequence of fatigue, and want of refreshment for several hours, the proportion of azote in the lungs might be greater.

Every 100 parts of oxygen, before it was inspired, contained 2.5 azote.

100 : 2.5 :: 3420 : 85.50;

Consequently it contained 85.50 cubic inches of azote.

From 263.10
Deduct 85.50 the original azote,

177.60 will be left for the increase of azote.

Then supposing as before, that the quality of the air in the lungs, before the experiment was 84 per cent. azote, 16 oxygen, and after the experiment 5.5 per cent. azote, 94.5 oxygen, as found in the last gasometer, we take

$\frac{84x}{100}$ azote at the beginning,

$\frac{5.5x}{100}$ azote at the end;

$$\frac{5.5x}{100} + 177.60 = \frac{84x}{100}$$

$$177.60 = \frac{84x}{100} - \frac{5.5x}{100} \text{ or } .84\,x - .055\,x = .785x,$$

$$x = \frac{177.60}{.785} \text{ or } 226 \text{ cubic inches.}$$

Hence it appears, that previous to the experiment, the lungs contained in this instance 226 cubic inches, and if we suppose them to be in the same state after, as before the experiment, the quality of the gas in each case will be as follows:

Contents of the Lungs before the Experiment.

189.84 cubic inches of azote,
36.16 oxygen,

226

Contents of the Lungs after the Experiment.

12.43 cubic inches of azote,
213.57 oxygen

226

Calculation for Oxygen.

3420 — 85.50 = 3334.50 original oxygen,
Add 36.16 in the lungs before the experiment,

3370.66 { total quantity of oxygen before the experiment.

After the Experiment.

100 : 66 :: 250 : 165 oxygen in No. 1. 165
100 : 82 :: 168 : 137.76 —— in No. 13 137.76
100 : 81.5 :: 2944 : 2399.36 —— in mixt. 2. to 12. 2399.36
—— in carbonic acid 396.78
—— in lungs after expt. 213.57

3312.47

3370.66 original oxygen,
3312.47 after experiment,

58.19 deficiency.

The observed deficiency in this experiment was 58.

The deficiency in this case, and in the former experiment with oxygen, though comparatively small, when contrasted with the quantity of gas respired, is larger than the average with atmospheric air; it seems probable, therefore, that a portion may be detained in the system. It must be remembered that what we call residual gas, is not only that contained in the substance of the lungs, and in its appendages, but also that contained in the fauces and mouth.

Eighteenth Experiment.

Barom.	Therm.	Time.	Cub. inches of oxygen gas inspired.	Cubic inches expired.	Deficiency.
30.15	70°	8′.45″	3130	3060	70.

The operator breathed as usual, after having made a strong effort to exhaust his lungs; his pulse before the experiment was 84, the thermometer under his tongue 98°: after the experiment his pulse was 96, and the thermometer under his tongue still 98°.; the same gentle glow and perspiration was felt as in the other experiments on oxygen; a portion of the gas was saved from each of the mercurial gasometers, and their amounts were as under:

No.	
1.	196
2.	228
3	284
4.	294
5.	248
6.	280
7.	258
8.	272
9.	250
10.	304
11.	223
12.	223
	3060

No. 1. tried by itself, contained in 100 parts,

9 carbonic acid,
22 azote,
69 oxygen,
100

No. 12, the last, contained in 100 parts,

12 carbonic acid,
5 azote,
83 oxygen,
100

On account of an accident we cannot give the proportions contained in 2 to 10; but the contents of the first and last gasometers confirm the former experiment, and shews that the proportion of azote continues to diminish, as the experiment proceeds, and also that there is a larger proportion of carbonic acid given off when oxygen is employed, instead of atmospheric air.

In this recital of experiments, which have occupied a considerable portion of time, and attention, we have endeavoured to give a plain statement of facts, from which every one may draw conclusions for himself; we shall here, however, take the liberty of briefly recapitulating the principal of those facts, and submitting what seems to us the most obvious inferences.

1. It appears that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed, and therefore there is no reason to conjecture that any water is formed by a union of oxygen and hydrogen in the lungs.

2. Atmospheric air once entering the lungs, returns charged with from 8 to 8.5 per cent. carbonic acid gas, and when the contacts are repeated almost as frequently as possible, only 10 per cent. is emitted.

The 12th and 13th experiments prove, that when the inspirations and expirations are more rapid than usual, a larger quantity of carbonic acid is emitted in a given time, but the proportion is nearly the same, or about 8 per cent. The proportions of carbonic acid gas, in the first and last portions of a deep inspiration, differ as widely as from 3.5 to 9.5 per cent.

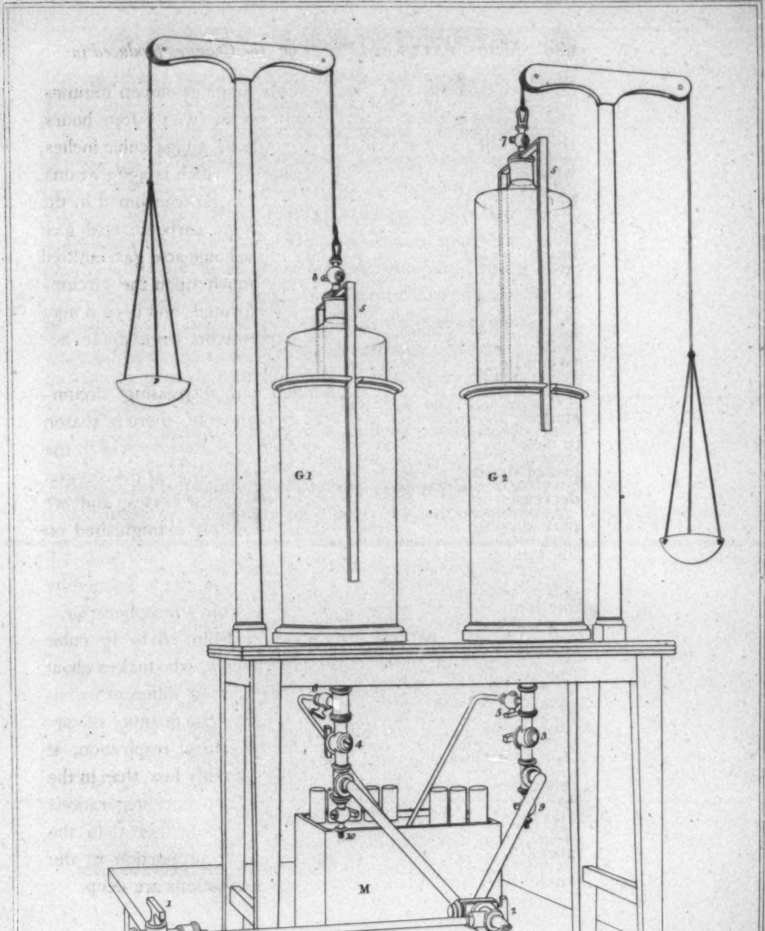
3. Considering the 11th as a standard experiment, it appears that a middle sized man, aged about thirty-eight years, and whose pulse is seventy on an average, gives off 302 cubic

inches of carbonic acid gas from his lungs in eleven minutes, and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains; the carbon, in which is 5363 grains, or rather more than 11 oz. troy, the oxygen consumed in the same time will be equal in volume to the carbonic acid gas, but it is evident, that the quantity of carbonic acid gas, emitted in a given time, must depend very much upon the circumstances under which respiration is performed; and here it may be proper to notice that all the experiments were made between breakfast and dinner.

4. When respiration is attended with distressing circumstances, as in the 14th and 15th experiments, there is reason to conclude that a portion of oxygen is absorbed; and in the last of these experiments, we may remark, that as the oxygen decreases in quantity, perception gradually ceases, and we may suppose that life would be completely extinguished on the total abstraction of oxygen.

5. A larger proportion of carbonic acid gas is formed by the human subject from oxygen, than from atmospheric air.

6. An easy, natural inspiration, is from 16 to 17 cubic inches in the subject of these experiments, who makes about 19 in a minute; this, however, will vary in different individuals, and perhaps we ought to estimate the quantity of carbonic acid gas, given off in perfectly natural respiration, at somewhat less, and most likely at considerably less, than in the statement above, when we consider that in short inspirations the quantity of air which has reached no farther than the fauces, trachea, &c. bears a much larger proportion to the whole mass respired, than when the inspirations are deep.



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7. No hydrogen, nor any other gas, appears to be evolved during the process of respiration.

8. The general average of the deficiency in the total amount of common air inspired, appears to be very small, amounting only to about 6 parts in 1000, and we are inclined to attribute it in great measure to the difficulty in exhausting the lungs as completely after an experiment as before it; the first expiration being made into the open air, the last into the apparatus.

9. The experiments upon oxygen gas prove that the quantity of air remaining in the lungs and its appendages is very considerable, and that, without a reference to this circumstance, all experiments upon small quantities of gas are liable to inaccuracy.

Other important conclusions might perhaps be drawn from the facts related in this paper, but having already trespassed largely upon the time of the Society, we shall abstain from any farther remarks, until we bring forward a new series of experiments.

XVIII. *Description of an Apparatus for the Analysis of the Compound Inflammable Gases by slow Combustion; with Experiments on the Gas from Coal, explaining its Application.* By William Henry, *M. D. Vice-Pres. of the Lit. and Phil. Society, and Physician to the Infirmary, at* Manchester. *Communicated by* H. Davy, *Esq. Sec. R. S.*

Read June 23, 1808.

The aëriform compounds of hydrogen and carbon, which were already entitled to accurate investigation, as objects of scientific research, have derived an additional claim to the attention of the chemist, from their application to an important economical purpose, described in a late communication to the Royal Society.* Yet there is, perhaps, no part of chemistry, the investigation of which is beset with greater difficulty, or with more numerous sources of error; insomuch, that the actual state of the science enables us to attain scarcely more than approximations to the truth, and degrees of probability of greater or less amount. It was the object of the experiments, which are described in the following pages, rather to remove some of the obstacles, which present themselves to a successful enquiry into the nature of these bodies, than to acquire such facts, as may enable the chemical philosopher to decide the controverted question respecting their composition. Results, sufficiently multiplied and precise for this purpose, would require a larger appropriation of time, than I have the prospect of being able to bestow; and I can only on the present occasion, offer an example of the method, in which it appears to me that the analysis of this class of substances will be most successfully attempted.

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* See Mr. Murdoch's paper, p. 124.

When a vegetable substance, composed (as may be assumed to simplify the statement) of oxygen, hydrogen, and carbon, united in the form of a ternary compound, is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities, which constituted the triple combination, is destroyed; and the elements, composing it, are united in a new manner. Those, which are disposed to enter into permanently elastic combinations, escape in the state of gas. The carbon, uniting with oxygen, either composes carbonic acid gas, or, stopping short of that degree of oxygenation, which is essential to change it into an acid, is converted into carbonic oxide. The hydrogen, combining with a portion of carbon, constitutes a binary compound of those two ingredients, forming either what has been called *carbureted hydrogen gas*, or *super-carbureted hydrogen*, better known by the appellation of *olefiant gas*. Towards the close of the process, a portion of simple hydrogen gas is also mingled with the products. Perhaps in no instance is any one of the gases, which have been enumerated, obtained perfectly pure, by the distillation of a vegetable substance. The aëriform fluids, which are thus generated, are found to be possessed of almost every degree of specific gravity; and to yield, by combustion, extremely different results, according to the temperature at which they have been formed; the stage of the process at which they have been separated; and other modifying circumstances. It becomes an interesting question, whether these gases, so much diversified in their physical and chemical properties, are mixtures of a few binary compounds, with which chemists are already acquainted; or whether, on the contrary, their elements are capable of uniting in indefinite proportions, and of composing ternary compounds of oxygen, hydrogen, and carbon, or varieties of *oxy-carbureted* hydrogen. It would encroach too much on the time of the Royal Society, to enter upon this controversy. And, as neither opinion admits, at present, of demonstrative evidence, I may be permitted, in explaining the following experiments, to assume that theory, which appears to me most probable; viz. that the aëriform products of the distillation of vegetable substances, are mixtures of carbonic acid, carbonic oxide, olefiant, carbureted hydrogen, and simple hydrogen gases; or of two or more of these in various proportions.

The analysis of these compound gases has hitherto been attempted solely by their rapid combustion with oxygen gas, in the following manner: a mixture of the inflammable gas with oxygen gas in known proportions, is admitted into a VOLTA's Eudiometer, inflamed over mercury by the electric spark; and the diminution ascertained. To the remainder caustic potash or lime water is added, by which it sustains a second diminution of bulk; and the amount of this denotes the quantity of carbonic acid formed by the combustion. The quantity of nitrogen gas, in the oxygen employed, as well as in the residue left by potash, being determined by a fit eudiometrical test, it is easy to infer what quantity of oxygen has been absorbed by the detonation. And as it is proved that oxygen gas sustains no change of bulk by conversion into carbonic acid, we may conclude that, after deducting from the volume of oxygen gas expended, that of the carbonic acid which has been formed, the remaining number shews how much oxygen has been employed in the saturation of hydrogen. If, for example, 100 measures of carbureted hydrogen consume 200 of oxygen gas, and give 100 of carbonic acid, it follows, that the carbonic acid holds in combination 100 measures of the oxygen gas consumed; and that the remaining hundred have been applied to the saturation of hydrogen. In this estimate it is assumed, that the carbon has acquired, by combustion, the whole of the oxygen necessary for its acidification, and that no part of it existed previously in the state of carbonic oxide, a proposition, in many cases, perhaps, very far from being consistent with the truth. This, however, admits of being decided by an accurate comparison between the weight of the gases consumed and that of the products.

For the purpose of obtaining a general approximation to the nature of a combustible gas, it may be sufficient to examine its coincidence with those, the properties of which have been already determined. The following table exhibits the results of the combustion of the few gases, that appear entitled to be considered as distinct species. They are deduced from the experiments of Mr. Cruikshank and Mr. Dalton.

Kind of Gas.	Sp. Grav. (air = 1000.)	100 measures,		
		Take meas. of oxygen.	Give carb. acid.	Are diminished by firing.
Olefiant	909	300	200	200
Carbonized hydrogen, from stagnant water,	600	200	100	200
Carbonic oxide	967	45	90	55
Hydrogen gas	84	50		154

The inflammability of the compound gases, and their fitness for the purpose of affording light, are directly proportionate to the quantity of oxygen required for their saturation. The olefiant gas, therefore, burns with the greatest brilliancy; carbureted hydrogen gas, though inferior, affords a dense and compact flame; but the carbonic oxide and hydrogen gas are entirely unfit to be employed as the means of artificial illumination.

In the execution of a series of experiments on the compound combustible gases, which are described in the 11th volume of Mr. NICHOLSON's Philosophical Journal, I had reason to be dissatisfied with the above method of effecting their decomposition, and to distrust the results which were obtained. The products of the combustion of the same gas varied considerably in different experiments; and, with respect to some, it was evident that the full proportion of their carbonaceous ingredient was not oxygenised, in consequence of the precipitation of charcoal in the act of detonation. The quantities, also, that can be submitted in this way to experiment, are extremely minute; and the inflammation of highly combustible gases is attended, as I have more than once experienced, with considerable danger from the bursting of the glass tubes. It was desirable, therefore, to employ a process not liable to these objections; and after many alterations of the apparatus, contrived with this view, I at length fixed upon one, which I shall now proceed to describe.

The principal parts of the apparatus, are two glass cylinders, or air receivers,* *b b* and *o o*, of which the larger one is intended to contain oxygen gas, and the smaller one, the inflammable gas submitted to experiment. They are connected by a bent glass tube *s s*, the diameter of which should not be less than $\frac{1}{10}$ of an inch, to the upper extremity of which is cemented an iron burner, *t*, the orifice of which is about $\frac{1}{30}$ of an inch, while to the lower end a socket is fixed, on which may be occasionally screwed the cock *r*. The receiver *o o* is contained in a larger glass jar *n n*, and is closed at the top by a brass cap *p*, and stop cock *q*. The oxygen gas receiver is, also, closed by a brass cap *e* and cock *f*, the lower orifice of which is tapped internally, for the purpose of receiving a small screw at the end of the copper wire *g*. This wire is in two parts, each of which screws into a moveable socket, connecting the two; and, by this contrivance, the wire may be lengthened or shortened at pleasure. To prepare the apparatus for use, the receiver *o o* is partly filled with the combustible gas; and is secured by wedges of cork *v v*, in the jar *n n*, the level of the water in the latter being regulated by opening the cocks *x* or *z*. The bent pipe *s s*, with its cock *r*, is screwed upon the top of the receiver, and partly immersed in the water of a pneumatic cistern, *a a*, so that the orifice of the burner may rise a few inches above the surface of the water. The receiver *b b* detached from the situation in which it is represented in the drawing, is then exhausted by an air pump; and, being filled with oxygen gas, is transferred (its mouth being closed during the act of removal with a piece of leather) to the cistern *a*, and quickly inverted over the burner *t*. By a little practice, this may be done with the admission of very little common air. A transferring vessel is then screwed upon the cock *f*; and a portion of oxygen gas removed for eudiometrical examination. To allow room for the expansion of the oxygen gas, the water is raised by a syphon to a proper height within the receiver *b*, as appears in the drawing.

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* I am indebted to Mr. H. CREIGHTON, of Soho, not only for a drawing of the apparatus but for much valuable assistance in the performance of the experiments.

The apparatus being thus disposed, the cock *f* is connected by the chain *h*, with the prime conductor of an electrical machine; and a rapid succession of sparks is made to pass between the copper ball at the end of the wire *g*, and the orifice of the burner. The cocks *q* and *r* being now opened, the stream of gas is kindled; and in order to prevent the flame from playing upon the wire, the jar *n n* is moved a little nearer to the cistern *a*, which brings the point of the burner into the axis of the receiver. At the same time, by opening the cock *x*, water flows into the jar *n n*, and finds its way into the receiver, through two small holes *w w* drilled near its mouth.

The combustion continues, until either the whole of the inflammable gas is consumed, or till the cocks *q* and *r* are shut. The wedges *v v* are removed; the receiver *o o* unscrewed; and the bent tube removed from its place. It is at this moment, that the cock *r* is useful, by preventing the escape of the gas from the receiver *b* through the tube *s s*. The upper part of the receiver is cooled by the application of a wet sponge. Without waiting, however, till the gas has attained the temperature of the atmosphere, a very small and sensible thermometer is introduced into it; and the height of the mercury is noted, as soon as it becomes stationary. The volume of the residuary gas is then observed, and is reduced by calculation, to the bulk which it would occupy at 60° of Fahrenheit. Either the whole, or an aliquot part of it, is removed by a transferring vessel, screwed upon the cock *f*, to a mercurial cistern, where the proportion of carbonic acid is determined by liquid potash. The proportions of oxygen and nitrogen gases, in the unabsorbed residue, are learned by agitation with sulphuret of lime, observing the precautions which have been stated by De Marti. The residuary oxygen being deducted from the quantity at the outset of the experiment, shews how much oxygen has been expended in the combustion of the inflammable gas. It is scarcely necessary to observe, that the gases are carefully reduced, at each stage of the operation, to a mean temperature and pressure, (60° of the thermometer, and 30 inches of the barometer).*

The process of combustion, as thus stated in general terms, appears sufficiently simple. It is often, however, rendered complicated by the imperfect combustion of the inflammable gas, a part of which escapes through the orifice of the burner, either wholly unaltered, or only partially burned. As this portion is not absorbed by sulphuret of lime, it gives a fallacious appearance of an actual addition of nitrogen to the oxygen gas remaining in the receiver *b*. I am unacquainted with any method of entirely obviating this difficulty; but its amount may be diminished by an attention to certain precautions. With this view, the pressure upon the gas, contained in the receiver *o o*, should, on first opening the cocks *q* and *r*, be no more than is sufficient for its gentle expulsion through the tube *s s*. When, however, the stream is once kindled, the larger the flame, and the more active the combustion, within certain limits, the more completely is the gas consumed. It is necessary, also, to stop the combustion, before it is rendered languid by the admixture of carbonic acid with the gas in the receiver *b*, and by the diminished purity of the oxygen gas. If this be not attended to, a large proportion of the inflammable gas, towards the close of the process, makes its escape unaltered into the receiver *b*. In general I have found, that setting out with oxygen gas of equal purity, the more combustible the inflammable gas submitted to experiment, the more complete is its decomposition by slow combustion. The apparatus, therefore, is better adapted to the analysis of olefiant gas, of carbureted hydrogen gas, or of mixtures of these two, than of carbonic oxide, or any gas of which that oxide forms a large proportion.

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* The rules observed in these calculations, are stated in my Epitome of Chemistry, 5th edition, p. 441.

The inflammable gas, which has found its way into the receiver *b*, is always present in too minute a quantity to compose, with the residuary oxygen, after the removal of the carbonic acid, a mixture capable of being inflamed by the electric spark. To ascertain its precise quantity, it is necessary to have recourse to another operation. After trying, eudiometrically, the quality of an aliquot part of the gas in the receiver *b*, let a similar aliquot part be deprived of its carbonic acid, and then mixed with a portion of pure hydrogen gas, not exceeding one third or one fourth the estimated bulk of the oxygen which it contains. Detonate the mixture, and observe the amount of the diminution after the explosion; the products of the combustion; and the quantity of oxygen gas consumed. After subtracting, from the total expenditure of oxygen, half the bulk of the added hydrogen gas, the remaining number shews how much oxygen has been absorbed by the combustible gas contained in the residue. By the rule of proportion, it may be determined, how much carbonic acid would have been produced, by the oxygenation of the whole of the combustible gas, and what quantity of oxygen it would have saturated.

The most obvious objection to this method of analyzing the compound gases is, that the real proportion of the products, resulting from their combustion, may perhaps be disguised, in consequence of the absorption of a part of the carbonic acid by the water, over which the experiment is made. By frequent trials, however, I find that this is a source of error too trivial to be deserving of consideration; and that the proportion of carbonic acid, thus generated, exceeds what is composed by the rapid combustion of the same gas over mercury. When the operator has acquired sufficient dexterity, the interval of time, between the completion of the combustion and the admeasurement of the residue, is too small to allow an absorption to any notable amount. It must be observed, also, that the carbonic acid constitutes only a small part of the residue; and is, for that reason, very little acted on by water, conformable to a principle which I have explained in the Philosophical Transactions for 1803, p. 274. I believe, therefore, that with an attention to those observances, which are required in all delicate experiments on gases, and to the changing circumstances of temperature and pressure, this apparatus is fully adequate to the purpose for which it is intended. It will be easy, however, for those who have the command of a sufficient quantity of mercury, to adapt the apparatus to that fluid. As an exemplification of the method of using it, in the simplest possible case, I shall state the results of the combustion of hydrogen gas.

At the outset of the experiment, there was contained in the receiver *o o*, a quantity of hydrogen gas, equal, when reduced to a mean temperature and pressure, to 15.8 cubic inches

Of these, there remained unconsumed	2.5	
Hydrogen gas burned - -	13.3	
In the receiver *b* were 49 cubic inches of oxygen gas, consisting of - -	33.5 oxygen,	15.5 nitrogen
At the close of the experiment, there remained, in *b*, 43.5 *c. i.* composed of - -	27.25	16.25
Cubic inches of oxygen gas consumed - - -	6.25	

But estimating from the first diminution (viz. 49—43.5) only 5.5 cubic inches of oxygen would appear to have been absorbed; and the nitrogen gas, by eudiometrical experiments, would seem to have been increased 0.75 of an inch. As the hydrogen gas, however, had been prepared from zinc and sulphuric acid with extreme caution, and did not contain an appreciable quantity of common air, no such addition of nitrogen could have taken place. The apparent increase, then, may be fairly imputed to the escape of 0.75 of an inch of hydrogen gas, which is to be deducted from the 13.3 cubic inches at the outset of the experiment; and hence the real quantity consumed will be 13.3 — 0.75 = 12.55. The true consumption, also, of oxygen gas was 5.50 + 0.75 = 6.25, or pretty exactly, as it ought to be, half the bulk of the hydrogen gas, which was actually burned.

An example of the analysis of a highly combustible species of elastic fluid is furnished by the following experiments on the olefiant gas, obtained from alcohol and sulphuric acid.

Of this gas 100 cubic inches, at a mean of the barometer and thermometer, were equal to 30 troy grains; hence its specific gravity was 967.

In the receiver *o o*, were contained of this gas	6.3	cub. in.
Residue	2	
Gas consumed - - - -	4.8	

In the receiver *b*, were 43.4 inches of oxygen gas. After the combustion, there remained 38.2 cubic inches of mixed gases, of which 8.6 were carbonic acid. None of the inflammable gas, which passed through the bent tube, had escaped being burned, for the quantity of gas in *b*, not absorbable by sulphuret of lime, so far from having been increased, was found to have sustained a trifling diminution. The oxygen gas, which was consumed, amounted to 13.8 cubic inches. Reducing these results to centesimal proportion, 100 cubic inches of this gas would give 200 of carbonic acid, and absorb 325 of oxygen gas. This experiment agrees with Mr. DALTON's, as to the proportion of carbonic acid from the combustion of olefiant gas, but assigns a larger consumption of oxygen. It may be observed, however, that the specific gravity of the gas, which I employed, exceeded a little the statement of the Dutch chemists, who found its specific gravity to be 909, common air being 1000.

Having satisfied myself, by repeated experiments, of the accuracy of the results which may be thus obtained, I proceeded to the combustion of the gases from a variety of vegetable substances, and especially from those which it seemed probable might become economical sources of light. In the present memoir, I shall describe those only, which were made on coal and a few similar substances, reserving the rest for a future communication.

Gas from Cannel Coal.

This was received in two separate portions. Of the first product, 100 cubic inches, corrected to a mean temperature and pressure, weighed 24.28 grains. Hence its specific gravity was to that of atmospheric air as 783 to 1000. The second product was much lighter, 100 inches weighing only 10.4 grains, and having, therefore, the specific gravity of 335. The results are comprehended in the following table. The carbonic acid, stated to have been generated by the second combustion, was formed by adding to an aliquot part of the residue, after the removal of the carbonic acid, a proportion of hydrogen gas; detonating the mixture by the electric spark; and proceeding as already directed. The two first lines contain the minutes of actual experiments; the third and fourth these results reduced to centesimal proportion.

Sp. Grav.	Cub. inches burned.	Oxyg. gas consumed.	Carb. acid. generated.	Carb. acid formed by second combustion.	Oxygen consumed by second combustion.	Total oxygen consumed.	Total, carb. acid formed.
783	7.3	16.5	8.3	1.9	0.9	17.4	10.2
335	9.8	9.4	4.8	0	0	9.4	4.8
783	100	222	113.7	2.6	12	234	139.7
335	100	96	49	0	0	96	49

The early product of the gas from cannel coal, before being washed with lime water or caustic potash, is a mixture of several different gases, viz. carbonic acid, sulphureted hydrogen, olefiant, and a fourth, which is either a gas *sui generis*, or a mixture of carbureted hydrogen, and carbonic oxide. To ascertain the proportion of these gases in any mixture, is a problem of some difficulty. Sulphureted hydrogen and olefiant gases experience, it is well known, an immediate condensation, when mingled with oxy-muriatic acid gas, and in this way they may be separated from carbonic acid. Again, sulphureted hydrogen and carbonic acid are absorbed by liquid potash, which has no action on olefiant gas. If, therefore, two equal portions of the gas from coal be mixed with oxy-muriatic gas, the one in its recent state, the other after being washed with potash, the condensation of the former will be found to exceed that of the washed portion. By the combined use of these agents, we may attain an approximation, at least, to the proportions in which carbonic acid, olefiant, and sulphureted hydrogen gas are mingled with the aëriform product of coal. The rule may be stated as follows; to a measured quantity of oxy-muriatic acid gas, contained in a graduated tube, add twice its bulk of the recent coal gas, and at the expiration of one or two minutes observe the diminution which has taken place. Wash an equal quantity with caustic potash; note the loss; and submit the residue to the action of oxy-muriatic acid as before. The second diminution, thus effected by oxymuriatic gas, divided by 2.2, gives the proportion of olefiant gas. Deduct this absorption from the first, and, dividing the remainder by 1.8, we obtain the quantity of sulphureted hydrogen. Lastly, to know the quantity of carbonic acid gas, substract, from the diminution effected by potash, the amount of the sulphureted hydrogen gas. An example, taken from actual experiment, will best explain the application of this rule.

One hundred measures of the first product of gas from cannel coal lost, by agitation with liquid potash, 9.7 measures.

The remainder, being mingled with one fourth its bulk of oxy-muriatic acid gas, the mixture lost 10.4 measures. This diminution, 10.4, divided by 2.2, gives 4.9 for the proportion of olefiant gas. But 100 measures of the unwashed gas sustained, by admixture with oxy-muriatic acid, a diminution of 20 measures. Now, deducting, from this diminution, that occasioned by the condensation of olefiant gas, (viz. 20 — 10.4,) there remain 9.6, which, divided by 1.8, gives 5.3 for the proportion of sulphureted hydrogen gas. And the diminution by potash (= 9.7) — 5.3 gives 4.4 for the proportion of carbonic acid gas. Hence 100 measures of the first product of gas from cannel coal contain,

1. Of inflammable gas, not affected by the foregoing agents - -	85.4
2. Of sulphureted hydrogen gas, -	5.3
3. Of olefiant gas - - -	4.9
4. Of carbonic acid gas - - -	4.4
	100

The proportion of common air, in the foregoing specimen of gas, and in all cases when care was taken to exclude it, was too small to deserve being taken into the account, not appearing, by the test of nitrous gas, to exceed 1 per cent.

The following table exhibits the composition of gas from various kinds of coal. In the last column, under the term inflammable gas, is comprehended that portion, which is neither suddenly condensed by oxy-muriatic acid gas, nor absorbed by potash. A name more descriptive cannot be applied to it, because it varies essentially in different cases, and the proportion of its components is still matter of doubt.

TABLE I.

Kind of Coal.	No. of the product.	One hundred measures consisted of			
		Sulph. Hydr.	Carb. acid.	Olefiant	Inflam.
Wigan cannel.	1	5.3	4.4	4.9	85.4
	2	0.	1.8	0.	98.2
Wednesbury, Staffordshire.	1	4.9	3 4	0	91.7
	2	0.	2.8	0	97.2
Newcastle on Tyne.	1	2.9	2.8	2.7	91.6
	2	2.2	1.7	0	96.1
Newcastle, Staffordshire.	1	3	2.7	0	94.3
	2	1.4	2	0	96.6
	3	0	1.4	0	98.6
Middleton, near Leeds.	1	3	2.	1	94
	2	1.4	1.7	0	96.9
	3	0	2	0	98.
Black Mine, near Manchester.	1	3.3	3.2	2.5	91
	2	2	1.1	0	96.9
	3	2	1.	0	97
	4	0.5	1.2	0	98.3
	5	0	1.2	0	98.8
	6	0	0	0	100
Merthyr, Glamorganshire.	1	1	1.7	0	97.3
	2	0	1.7	0	98.3
	3	0	1.6	0	98.4
	4	0	1.5	0	98.5
	5	0	1.	0	99
	6	0	0	0	100
Native coal tar.	–	13	6	15	66
Caoutchouc.	–	0	4.9	17	78.1

After separating the sulphureted hydrogen and carbonic acid gases by agitation with liquid potash, the residue, consisting of the inflammable gas mixed with the proportion of olefiant gas produced along with it, was submitted to combustion. The following table shews the average results of a number of these experiments.

TABLE II.

Kind of Coal.	No. of the product.	Weight of 100 cubic inches (Ther. 60°. Bar. 30.)	Spec. Grav. (Air 1000.)	100 cubic inches	
				consume oxyg. gas.	give carbonic acid.
Wigan cannel,	1	24.28 Grs.	783	234	139.7
	2	10.4	335	96	49
Wednesbury coal,	1	20.9	674	190	97.5
	2	9.8	316	85	46
Newcastle on Tyne,	1	19.3	622	190	100
	2	9.8	316	86	45
Newcastle Staffordshire,	1	19.6	632	195	98
	2	17.7	570	165	80
	3	12.1	390	100	60
Leeds,	1	20.7	670	190	100
	2	15.1	487	lost by accident.	
	3	9.8	316	85	42
Black-Mine, Lancashire,	1	19.4	627	186	97
	2	15	484	137	65
	3	11.3	364	100	50
	4	10	322	90	47
	5	9.5	307	85	45
	6			80	40
Merthyr,	1	12	387	117	62
	2	9.5	307	90	47
	3	8	261	75	39
	4	5.9	190	60	31
	5	5.8	187	57	26
	6	5.5	177	50	20
Coal tar,	–	24.2	780	233	150
Caoutchouc.	–	—	—	204	121

An attentive examination of the results, contained in both the tables, suggests the following general remarks.

1. The olefiant gas is a very sparing product of the distillation of pit-coal. It is found only in the first portions, and even of these it does not compose more than 5 per cent. Its quantity, however, is very much influenced by the temperature employed. This remark, indeed, may be extended to all the aëriform products of coal; insomuch that from equal weights of the same coal it is difficult to obtain by different operations conducted on a small scale, products which are the

same either in quantity or quality. The gas from *Coalbrookedale* tar, and that from *Caoutchouc*, have a larger proportion of olefiant gas, which in them amounts to about one-sixth their bulk.

2. Sulphureted hydrogen gas is, also, most abundantly produced at the early stages of the distillation. Its proportion then varies from 1 to 5 per cent; and towards the close of the process it disappears entirely. It increases the illuminating power of the coal gas; but is by no means a desirable product; since it yields by combustion, a gas (the sulphurous acid) which is extremely offensive and irritating to the lungs. By the distillation of coal, more sulphureted hydrogen is proproduced, than is discovered among the aëriform products; for a part, uniting with the ammonia which is generated at the same moment, forms sulphuret of ammonia, a compound which I have found among the condensed products.

3. Carbonic acid gas, like the two preceding ones, appears only at an early stage of the process, and in small proportion, never amounting to 5 per cent. A portion of this gas, also, unites with ammonia, and hence carbonate of ammonia is found in the condensed fluid.

4. The gas from coal undergoes a gradual diminution of specific gravity and combustibility, from the commencement to the close of the process. This is best shewn by inspecting the results of the experiments on the *Black-Mine* and *Merthyr* coal gas in Table II. because they were reserved in a greater number of separate portions than usual. The progression would, perhaps, have been more regular, in these as well as in the other instances, if much of the gas had not been allowed to escape, in consequence of the immense quantity

which was produced. The specific gravity of the coal gas appears to afford a measure of its fitness for illumination, sufficiently accurate for practical uses; but does not bear an exact correspondence to the chemical properties of the gas, as ascertained by combustion. It may be remarked, also by comparing the two last columns of the second table, that the carbonic acid produced does not always bear the same proportion to the oxygen expended. Thus the first product of gas from cannel coal combines with 234 measures of oxygen gas; and gives 139.7 of carbonic acid. But the gas from coal tar, with only an equal consumption of oxygen, yields 150 measures of carbonic acid.

5. The aëriform product of coal does not precisely answer to the characters of any one of the combustible gases, with which we are acquainted. The first product, however, of the distillation of common pit coal, after being washed with potash, approaches very nearly in its properties to carbureted hydrogen gas. The gases, which surpass this in specific gravity, are mixtures of carburetted hydrogen with olefiant gas, and perhaps a small proportion of carbonic oxide. The lighter gases, in addition to carbureted hydrogen, probably contain a variable proportion of hydrogen gas and a small quantity of carbonic oxide. The extreme levity of some of the products, especially of the gas from *Merthyr* coal, cannot be explained on any other supposition.

6. The products of the combustion of a cubic foot of coal-gas, of medium quality, viz. of the specific gravity 622, (such as the first products from *Newcastle on Tyne* coal) may be stated as follows:

	Grains.
A cubic foot, at a mean of the barometer and thermometer, - - - -	333.5
By combustion, it yields 817.3 grains of carbonic acid, the carbon in which may be estimated * at	233.7
Grains of hydrogen in a cubic foot of coal gas	99.8

But 99.8 grains of hydrogen are equivalent to the saturation of 554.9 grains of oxygen, with which they form 654.7 grains of water. Hence the oxygen consumed ought from calculation to be $817.3 - 233.7 = 573.6 + 554.9 = 1128$

And the quantity actually consumed appears by experiment to be - - -	1110.3
Error	17.7

The difference, in this example, between experiment and calculation is not greater, than, in such delicate processes, may always be expected. A part of the deficiency in the oxygen actually consumed may be ascribed, also, to a small portion of the inflammable gas being already in the state of carbonic oxide.

Without repeating the particulars of a similar calculation made on gas of inferior quality, I shall annex a comparative statement of the specific gravities and composition of the good and inferior gases.

Source of the Gas.	Weight of a Cubic Foot.	A Cubic Foot consists of Carbon.	Hydr.	Oxygen Gas consumed by a Cubic Foot.	Gives Carb. Acid	Water
Newcastle coal	333.5gr.	233.7	99.8	1110.3	817.3	621
Ditto, last product	169.3	111.5	57.8	560.	400	384.9

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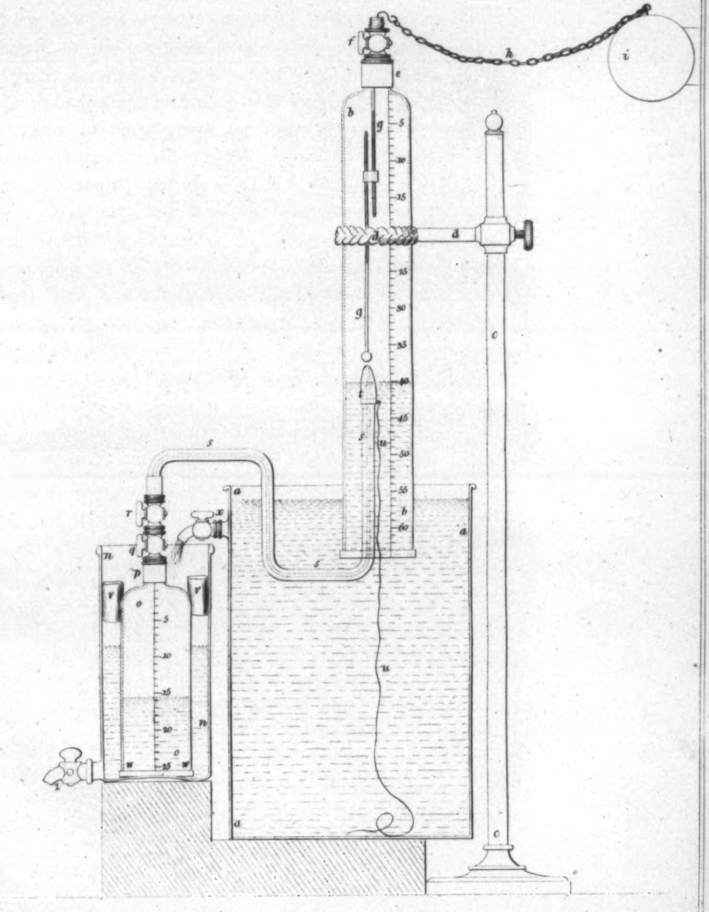
* Assuming the carbon to be 28.6 grains in 100 grains of carbonic acid, as is satisfactorily proved by the experiments of Messrs. ALLEN and PEPYS.

The inferior gas, also, probably contains carbonic oxide; for the quantity of oxygen gas, actually consumed, will be found, on calculation, less than it ought to be, if the carbon were not already combined with a portion of oxygen.

The quantity of water, which was generated by combustion, was not determined experimentally, but is merely estimated. It must be acknowledged that the decomposition of the inflammable gases cannot lead to unquestionable results, until the proportion of water, produced by their combustion, be also accurately ascertained. With the view of effecting this, I have already spent much time, and employed many contrivances, none of which have satisfactorily answered the purpose for which they were intended.

7. There appears to be a considerable difference in the specific gravity and combustibility of gas from various specimens of coal, even when taken at similar periods of the distillation. The coal from Merthyr in South Wales, which burns without flame or smoke, yields a gas which contains, in an equal volume, scarcely half as much combustible matter as the gas from Wigan cannel. This will probably be found to be the case with respect to all coal of similar quality, among which may be reckoned the Kilkenny coal. The most important difference among the varieties of this mineral, connected with their application as sources of light, consists in the quantity of sulphureted hydrogen gas, which is mixed with their aëriform products; and it unfortunately happens that the coal, otherwise best adapted to this purpose, yields generally the largest proportion of this offensive gas. The only effectual method of purifying the coal gas from sulphureted hydrogen, on the large scale of manufacture, will probably be found to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consist in agitation with quicklime and water, composing a mixture of the consistence of cream. Simple washing with water by no means effects the complete separation.

In the experiments which were made on the products of the distillation of coal, I purposely neglected the amount and analysis of the condensible fluids, because they cannot be advantageously ascertained by the same operation with the elastic ones. They may also be much better determined on the large scale of manufacture, than by limited experiments. For the same reason I was not solicitous to measure even the aëriform fluids; and on this subject, I believe, more accurate information has been communicated by Mr. MURDOCH, than it was in my power to acquire.

Manchester, May 19, 1808.

XIX. *An Account of some Peculiarities in the anatomical Structure of the Wombat, with Observations on the female Organs of Generation. By* Everard Home, *Esq. F. R. S.*

Read June 23, 1808.

A MALE wombat was brought from the islands in Basse's Straits, by Mr. BROWN, the naturalist attached to Captain FLINDERS's voyage of discovery. It was entrusted to my care, and lived in a domesticated state for two years, which gave me opportunities of attending to its habits.

It burrowed in the ground whenever it had an opportunity, and covered itself in the earth with surprising quickness. It was quiet during the day, but constantly in motion in the night: was very sensible to cold; ate all kinds of vegetables; but was particularly fond of new hay, which it ate stalk by stalk, taking it into its mouth like a beaver, by small bits at a time. It was not wanting in intelligence, and appeared attached to those to whom it was accustomed, and who were kind to it. When it saw them, it would put up its fore paws on the knee, and when taken up would sleep in the lap. It allowed children to pull and carry it about, and when it bit them did not appear to do it in anger or with violence. It appeared to have arrived at its full growth, weighed about twenty pounds, and was about two feet two inches long.

The koala is another species of the wombat, which partakes of its peculiarities. The following account of it was sent to me some years ago by Lieut. Colonel Paterson, Lieutenant-Governor of New South Wales. The natives call it the koala wombat; it inhabits the forests of New Holland, about fifty or sixty miles to the south-west of Port Jackson, and was first brought to Port Jackson in August, 1803. It is commonly about two feet long and one high, in the girth about one foot and a half; it is covered with fine soft fur, lead coloured on the back, and white on the belly. The ears are short, erect, and pointed; the eyes generally ruminating, sometimes fiery and menacing; it bears no small resemblance to the bear in the fore part of its body; it has no tail; its posture for the most part is sitting.

The New Hollanders eat the flesh of this animal, and therefore readily join in the pursuit of it; they examine with wonderful rapidity and minuteness the branches of the loftiest gum trees; upon discovering the koala, they climb the tree in which it is seen with as much ease and expedition, as an European would mount a tolerably high ladder. Having reached the branches, which are sometimes forty or fifty feet from the ground, they follow the animal to the extremity of a bough, and either kill it with the tomahawk, or take it alive. The koala feeds upon the tender shoots of the blue gum tree, being more particularly fond of this than of any other food; it rests during the day on the tops of these trees, feeding at its ease, or sleeping. In the night it descends and prowls about, scratching up the ground in search of some particular roots; it seems to creep rather than walk: when incensed or hungry, it utters a long shrill yell and assumes a fierce and menacing look. They are found in pairs, and the young is carried by the mother on its shoulders. This animal appears soon to form an attachment to the person who feeds it.

A specimen of this animal has since been sent to me in spirits; the viscera had been removed, but the male organs of generation, and the structure of the limbs, were the same as in the wombat. There was no subdivision of vessels in the groin as in the tardigrade animals.

The external form of the wombat has been described by M. Geoffroy in the second volume of the Annales du Museum National de France; and several parts of its internal structure have been taken notice of by M. Cuvier in his Leçons d'Anatomie comparée. It only remains to mention such peculiarities as have either been slightly touched upon, or entirely passed over in the different accounts. Among these is the mechanism of the bones and muscles of the hind legs, which differs in many respects from that of all other animals, except the koala. The following account of it is drawn up at my desire, by Mr. Brodie, from an accurate examination of the parts.

"There is no patella; but the tendon of the extensor muscles of the leg, where that bone is usually situated, is much thickened.

"The fibula is proportionably larger than in most animals. At the upper extremity it is broad, and has two distinct articulating surfaces: the anterior of which is articulated to the tibia, and the posterior to a small bone of a pyramidal shape, which is connected to the tendon of the external head of the gastro-cnemius muscle like a sesamoid bone. The lower extremity of the fibula is large, and forms about half of the articulating surface for receiving the tarsus at the ankle. An inter-articular cartilage is here interposed between the tibia and the fibula, and another between the fibula and the tarsus.

"The fibula has a slight degree of motion on the tibia at its upper end, and a half rotatory motion on it at its lower end. Between the two bones is a strong muscle, which passes from one to the other throughout their whole length. The fibres have their origin from the inner edge of the fibula, and pass obliquely inward and downward to be inserted into the opposite surface of the tibia. When this muscle contracts, it pulls the fibula forwards, and produces a degree of rotation on the tibia, which turns the toes inwards. The anterior surface of the muscle is covered by a thin fascia or interosseous ligament, and there is another fascia less complete on its posterior surface. The muscle of the leg, corresponding to the biceps flexor of the human subject, is inserted into the posterior part of the fibula, and is an antagonist to the muscle just described. Its action brings the toes back to a straight line, but does not turn them outwards."

This mechanism is met with in two animals, whose mode of life is very different, the one living on trees, the other not; but as they both burrow in the ground during the night, its use appears to be for throwing back the earth while the animal is burrowing. There is nothing at all similar to it in the hind legs of the mole, or other burrowing animals.

The internal structure of the stomach of the wombat resembles very closely that of the beaver, and is shewn in the annexed drawing. This is so different from that of the kanguroo, and all the other animals of the opossum tribe, that it forms a very extraordinary peculiarity.

The male organs of generation have been described by

CUVIER, with the exception of the prostate gland, which was supposed to be wanting. That distinguished anatomist could only have been led to this supposition from the parts in the specimen dissected being in an unfavourable state for examination. This gland resembles exactly that of the kanguroo, and is proportionably large.

The female organs have not been before described. The following description of them is particularly interesting, as the parts were in a state of impregnation, and they were examined immediately after the animal's death in New South Wales by Mr. BELL, a surgeon whose anatomical knowledge qualified him for the task. He unfortunately died soon after at Bombay. He sent the description to Sir JOSEPH BANKS, through whose kindness I received it.

Mr. BELL's *Description of the female Organs of the Wombat.*

"On laying open the cavity of the pelvis, I was surprised by the appearance of a double uterus, each of them distended into a pyramidal form; that of the right side was considerably the largest, and was about the size of a pullet's egg. From the fundus of each there was a Fallopian tube nearly three inches long terminating at the ovarium.

"The double uterus had one common neck half an inch long, and of considerable breadth and thickness: two lateral canals rose from the common neck on its posterior surface near its junction to the uteri, one on each side; they were about two inches long, having a semi-circular course, and terminated obliquely in the vagina.

"The uteri and their appendages were dissected out from the body, and examined more particularly. On opening the vagina, it was found to terminate at the common neck of the uteri, on each side of which were the openings of the lateral canals, and in the middle between them the meatus urinarius, with a slender fleshy pedicle on each side of it. Behind the meatus urinarius were two orifices leading to the two uteri, but they were filled with a thick gelatinous substance which rendered them completely impervious.

"I made a longitudinal incision into the largest of the uteri, and found its coats lined with the same jelly met with in its os tincæ. Continuing the incision through this jelly, and at the same time using gentle pressure, there issued a quantity of a thin pellucid fluid, accompanied by an embryo wrapped up in very fine membranes, which contained some of the same transparent fluid. The membranes did not appear to be at all connected by vessels either to the uterus or gelatinous matter. I had no doubt of the other uterus containing a similar embryo in a less advanced state.

"Each uterus had evidently only one Fallopian tube and one ovarium; this tube had no swell or enlargement where it joins the uterus, as in the kanguroo. In both ovaria were appearances like corpora lutea, but that of the right side was most distinct. The lateral canals opened into the uteri just where the neck terminated in the cavity. I have omitted to note how far these canals were pervious."

This account of Mr. Bell's, which differs so materially from that of the female organs of the kanguroo, corresponds exactly with that of the American opossum, only the parts are upon a larger scale; and it is found that the male organs of the wombat and koala are also similar to those of the

American opossum. These animals, therefore, form the intermediate link between the opossum and kanguroo.

These facts throw considerable light upon the mode of propagation of this very curious tribe of animals; and they confirm in the most satisfactory manner, the observations contained in a former paper upon the kanguroo, taken from a specimen which was sent to England preserved in spirits, and which was in an impregnated state, but the parts rendered very indistinct by being coagulated and long kept. In that instance the embryo was evidently surrounded by a coagulated jelly, and there was no connection between the fœtus and the coats of the uterus. Further evidence was however required upon so new and interesting a subject; this is now obtained by Mr. BELL's examination of the impregnated uteri of an animal whose mode of generation is the same, made immediately after death.

By comparing the male and female organs of the kanguroo, wombat, and American opossum, it appears evident that the semen of the male is carried directly into the cavity of the uterus through the os tincæ, and not by the lateral canals, as has been generally believed. The proofs of this are the following: the number of lateral canals opening into the vagina is the same in all these three animals, but in the kanguroo there is only one os tincæ; in the wombat and American opossum there are two; the male kanguroo has a pointed single glans penis, while the wombat and American opossum have a bifid glans penis with a double orifice; and these two orifices in the state of erection of the penis, do not diverge from one another so as to apply themselves to the oblique openings of the lateral canals, but oppose the two ora tincæ, whose situation is well adapted to receive them.

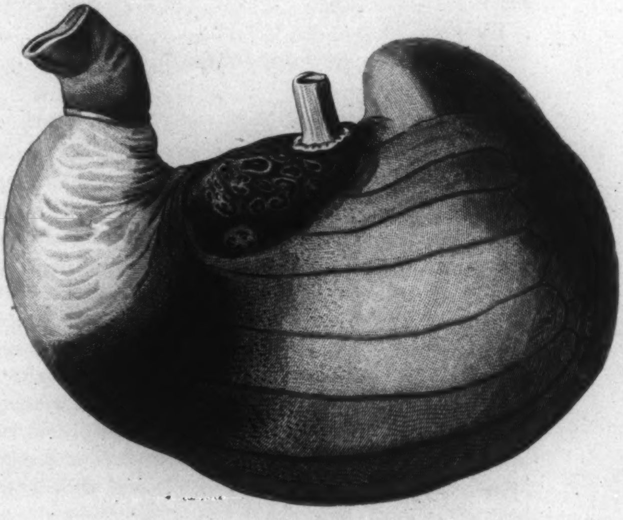
When once it is established, that the fœtus is nourished without a placenta, and that the semen is carried directly into the os tincæ, we are led to enquire where this gelatinous matter for the nourishment of the fœtus is formed; it cannot be supposed to be secreted from the internal membrane of the uterus itself, which is constantly undergoing a change to adapt it to the encreased bulk of the fœtus as well as of the jelly surrounding it. In birds, the albumen of the egg, which corresponds with this jelly, is formed in a tube adapted to that purpose called the oviduct, and these lateral canals, which open into the cavity of the uterus, bear a near resemblance in their form to the oviducts of birds; in the kanguroo they are found in the impregnated state to be enlarged, and to have a very free communication with the uterus, while the extremity next the vagina is rendered impervious; both of which circumstances induce me to believe that their sole use is to form this jelly, and to deposit it in the uterus.

In the bird, the yolk of the egg is the part first formed, and in its passage from the place of its formation, along the canal of the oviduct, the albumen is added to it, and the egg is completely formed before it receives the influence of the male; but in all the different genera of the opossum tribe impregnation takes place in the same manner as in the quadruped, the small vesicular ovum receiving the influence of the male at the ovarium; it then passes into the uterus, where it is deposited; but instead of becoming attached to the coats of the uterus by means of a placenta, as in other quadrupeds, the gelatinous matter from the lateral canals every where surrounds it, and supplies the embryo with nourishment.

EXPLANATION OF THE PLATE.

The stomach of the male wombat inverted immediately after death and distended with air, so as to expose completely its internal surface, in which the glandular structure on the small curvature is exactly similar to that of the beaver.

The parts are of the natural size.



Clift del. *Basire sc.*

XX. *On the Origin and Office of the Alburnum of Trees. In a Letter from* T. A. Knight, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read June 30, 1808.

My Dear Sir,

In my last communication I endeavoured to prove that the bark of trees is not subsequently transmuted into alburnum; and if the statements that I have there given be correct, they are, I conceive, decisive on the point for which I contended: and if the bark be not converted into alburnum, the experiments of Duhamel, and subsequent naturalists, and those of which I have given an account in former memoirs, afford sufficient evidence that the bark deposits the alburnous matter. If the succulent shoot of a horse chesnut, or other tree, be examined, at successive periods in the spring, it will be seen that the alburnum is deposited, and its tubes arranged, in ridges beneath the cortical vessels; and the number of these ridges, at the base of each leaf, will be found to correspond accurately with the number of apertures through which the vessels pass from the leaf-stalks into the interior bark, the alburnous matter being apparently deposited (as I have endeavoured to prove in former memoirs) by a fluid which descends from the leaves, and subsequently secretes through the bark.* I shall therefore venture to conclude that it is thus deposited, and shall proceed to enquire into the origin and office of the alburnous tubes.

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* Phil. Trans. 1801, p. 336.

The position and direction of these tubes have induced almost all naturalists to consider them as the passages through which the sap ascends; and at their first formation, when the substance which surrounds them is still soft and succulent, they are always filled with the fluid, which has apparently secreted from the bark. They appear to be formed in the soft cellular mass, which becomes the future alburnum, as receptacles of this fluid, to which they may either afford a passage upwards, or simply retain it as reservoirs, till absorbed, and carried off, by the surrounding cellular substance. The former supposition is, at first view, the most probable; but the latter is much more consistent with the circumstances that I shall proceed to state.

Many different hypothesis have been offered by naturalists to account for the force with which the sap ascends in the spring; of these hypotheses two only appear in any degree adequate to the effects produced. SAUSSURE, jun. supposes that the tubes contract as soon as they have received the sap in the root, and that this contraction, commencing in the root, proceeds upwards, impelling the sap before it; and I have suggested that the expansion and contraction of the compressed cellular, or laminated substance (the tissue cellulaire of DUHAMEL and MIRBEL) which expands and contracts with change of temperature * after the tree has ceased to live, might produce similar effects by occasioning nearly a similar motion and compression of the tubes, the coats of which are, I believe, universally admitted not to be membranous. But both these

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* Phil. Trans. 1801, p. 345.

hypotheses are inconsistent with the facts that I have now the pleasure to communicate to you.

Selecting parts of the stems of young trees, from which annual branches had sprung in the preceding year, I ascertained by injecting coloured infusions into the stems, through the annual shoots, that the tubes which descended from the latter, were, at their bases, confined to that side of the stem from which they sprang, and to the external annual layer of wood. Deep incisions were then made into the stems of other trees immediately beneath the bases of similar annual shoots, by which I am quite confident that all communication through the alburnous tubes, with the stem, was wholly cut off: yet the sap passed into the annual shoots in the succeeding spring, all of which lived, and some grew with considerable vigour. I, at the same time, selected many lateral branches, about three lines in diameter, in a nursery of apple trees, which I could easily secure to the stems of the adjoining trees to prevent their being broken. I then made an incision, more than two lines deep in each, on one side, and at the distance of six or seven lines another incision, equally deep, on the opposite side; and as I am quite certain, from the texture of these branches that the alburnous tubes passed straight through them, I am equally certain that every alburnous tube was at least once intersected. Yet the sap passed into these branches, and their buds unfolded in the succeeding spring, the incisions having been made in the winter. But I have repeated the same experiment after the leaves have been full grown in the summer, and still the branches have continued to live.

All naturalists have agreed in stating that trees perspire most in the summer, when their leaves have attained their full growth, and of course that much sap must ascend at this period; yet at this period the tubes of the alburnum appear dry, and to contain air only; which induced GREW to suppose that the sap rose in the state of vapour; a supposition by no means admissible. Yet it is, I conceive, evident that the sap can not rise, as a liquid, through dry tubes, nor in any state through intersected tubes; and therefore it appears probable that it does not rise at all through the tubes of the alburnum, and that those tubes are intended to execute a different office.

If the sap do not rise through the tubes of the alburnum, it must rise through the cellular substance; yet the passage of any fluid through this has been denied by almost every naturalist, probably because coloured infusions have not been observed to penetrate it, and because many naturalists have considered it as mere compressed medulla. MIRBEL, however, contends that the fluid which generates the new bark exudes from it; and although a fluid, capable of producing the same effects, exudes from the bark, when detached from the alburnum, I am much disposed to coincide with him in opinion, having observed a new bark to be generated on the surface of the cellular substance of pollard oaks, in detached spaces.* And if the sap in sufficient quantity to generate a new bark can pass through the cellular substance of an oak, it appears possible at least that the whole of the sap may ascend through it. Coloured infusions do not, I think, in any degree, pass through the bark of trees, yet it is evident that the sap passes readily through it; and therefore, should it be proved that such infusions do not penetrate the cellular substance of the alburnum, the evidence which this circumstance would afford would be very defective.

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* Phil. Trans. 1807, p. 7.

Amongst other experiments that I made to ascertain whether the cellular substance of the alburnum would imbibe coloured infusions, I took off branches of two years old with the annual shoots and leaves attached to them, in the summer, from trees of different species; and I effectually closed the alburnous tubes with a composition formed of calcined oyster shells and cheese,* and this was covered with a mixture of bees wax and turpentine, so as to effectually exclude all moisture. A part of the bark was taken off each branch, in a circle round it, a few lines distant from its lower end, where the tubes had been closed; and each branch was then placed in a decoction of logwood, in a vessel deep enough to cover the decorticated spaces. At the end of twenty hours, or somewhat longer periods, these branches were examined, and the coloured infusion was found to have insinuated itself between the alburnous tubes, in many instances apparently through the cellular substance. This was most obvious in the walnut tree, the young wood of which is very white. The principal object I had in view in making this experiment, was to detect the passages through which I conceived the sap to pass from the bark into the alburnum.†

From the preceding circumstances, I am disposed to infer that the sap secretes through the cellular substance of the alburnum; and through this I conceive that it must ascend when the tubes were intersected in the preceding experiments, and in those seasons of the year when the alburnous tubes are empty, though the sap must be rising with great rapidity: and I shall endeavour to shew that the presence of the sap in the alburnous tubes, during that part of the year in which trees, when wounded, bleed abundantly, does not afford any decisive evidence of the ascent of the sap through those tubes.

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* I have found this composition, and this only, to be capable of instantaneously stopping the effusion of sap from the vine, or other tree, in the bleeding season.

† Phil. Trans. 1807, p. 7.

In the last spring, when the buds of the sycamore first began to prepare for unfolding, I found that the sap abounded in the points at the annual branches; and at the same time it flowed abundantly from incisions made into the alburnum near the root. But when similar incisions were made at the distance of eight or ten feet from the ground, not the least moisture flowed; and the tubes of the alburnum appeared to contain air only. I also observed that the sap flowed as abundantly from the upper as from the under side of the lower incisions, if not more abundantly, and so it continued to flow to the end of the bleeding season.

The sap must therefore have been, by some means, thrown into the tubes above the incisions, for the quantity discharged from them exceeded more than a hundred times that which the tubes could have contained at the time the incisions were made, even had every tube been filled to the extremity of the most distant branch. And, as it has been shewn that the sap can pass up when all the alburnous tubes are intersected, there appears, I think, sufficient evidence that it must in this case have been raised by some other agent than those tubes.

Through the cellular substance I therefore venture to conclude that the sap ascends, and it is not, I think, difficult to conceive that this substance may give the impulse with which the sap is known to ascend in the spring. I have shewn that the bark more readily transmits the descending sap towards the roots than towards the points of the branches;* and if the cellular substance of the alburnum expand and contract, and be so organised as to permit the sap to escape more easily upwards from one cell to another, than in any other direction, it will be readily impelled to the extremities of the branches: and I have shewn that the statement, so often repeated in the writings of naturalists, of a power in the alburnum to transmit the sap with equal facility in opposite directions, and as well through inverted cuttings as others, is totally erroneous.†

If the sap be raised in the manner I have suggested, much of it will probably accumulate in the alburnum in the spring; because the powers of vegetable life are, at that period, more active than at any other season; and the leaves are not then prepared to throw off any part of it by transpiration. And the cellular substance, being then filled, may discharge a part of its contents into the alburnous tubes, which again become reservoirs, and are filled to a greater or less height, in proportion to the vigour of the tree, and the state of the soil and season: and if the tubes which are thus filled be divided, the sap will flow out of them, and the tree will be said to bleed. But as soon as the leaves are unfolded, and begin to execute their office, the sap will be drawn from its reservoirs, and the tree will cease to bleed, if wounded.

The alburnous tubes appear to answer another purpose in trees, and to be analogous, in some degree, in their effects, to the cavities in the bones of animals; by which any degree of strength, that is necessary, is given with less expenditure of materials, or the incumberance of unnecessary weight; and the wood of many different species of trees is thus made, at the same time, very light, and very strong, the rigid vegetable fibres being placed at greater distances from each other by the intervention of alburnous tubes, and consequently acting with greater mechanical advantage, than they would if placed immediately in contact with each other.

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* Phil. Trans. 1804, p. 5.

† Ibid.

I have shewn in a former communication, that the specific gravity of the sap increases during its ascent in the spring, and that saccharine matter is generated, which did not previously exist in the alburnum, nor in the sap, as it rose from the root: and I conceive it not to be improbable, that the air contained in the alburnous tubes may be instrumental in the generation of this saccharine matter. For I discovered in the last autumn, that much air is absorbed, or at least disappears, during the process of grinding apples for the purpose of making cider, and that during this absorption of air, the juice of acid apples becomes very sweet, and acquires many degrees of encreased specific gravity; and a similar absorption of air, with corresponding effects, is well known to take place in the process of malting.

I shall conclude with observing, that in retracting the opinion I formerly entertained respecting the ascent of the sap in the alburnous tubes, I do not mean to retract any opinion that I have given in former communications respecting the subsequent motion of the sap through the central vessels, the leaves, and bark; or the subsequent junction of the descending with the ascending current in the alburnum: every experiment that I have made has, on the contrary, tended to confirm my former conclusions.

I am,

My dear Sir,

Your much obliged, obedient Servant,

THOMAS ANDREW KNIGHT.

Elton, June 15, 1808.

XXI. *Eclipses of the Satellites of Jupiter, observed by* John Goldingham, *Esq. F. R. S. and under his Superintendance, at Madras, in the East Indies.*

Read June 30, 1808.

The following eclipses of the satellites of Jupiter, were observed with achromatic telescopes, by Dollond, of three and half feet focal length, and magnifying power between 70 and 80; having been constructed more immediately for this purpose, for which they were exceedingly well calculated in all respects.

An astronomical clock, with gridiron pendulum, and dead beat, regulated by transits of the sun and stars, was used for the time; which was deduced from the transit of the sun nearest the eclipse, and verified by the one immediately preceding or following.

The circumstances under which the eclipses were observed are noted; from these may be inferred, how far the results are to be depended upon: those observed with the "air clear and the planet high," are the most satisfactory and valuable, nothing to the contrary being afterwards expressed.

The longitude of the place, by numerous observations of various descriptions, is $5^h : 21' : 14''$ or $80° : 18' : 30''$ east of Greenwich: by comparing this with the numbers in the last column, the difference of the tables will be obtained.

The greater number of these eclipses were not visible at Greenwich, but have been found very useful, when correspondent observations have been taken in India.

Persons not much in the habit of observing these eclipses, but desirous of obtaining as much correct information from their observations as possible, may find the following general remarks of use.

A correct difference of longitude, it would appear, is not to be expected, by comparing the time of observation with that in the tables; it therefore becomes necessary to have a correspondent observation to compare with, or some satisfactory observations taken under a known meridian, about the time; from which the errors of the tables may be found. Correspondent observations, should, however, be obtained if possible: but it must not be supposed, that even these will give a correct difference of longitude, unless observed at both places, under the like favourable circumstances, and with telescopes of the same powers.

The air being clear; the planet so high as to be out of the thick atmosphere, and to make the position easy; the telescope sheltered from the wind, and steady; neither moonlight nor twilight, and the satellite not near the body of the planet: An eclipse observed under such circumstances, will, I apprehend, be as perfect as it well can be; and a correspondent observation taken under the like circumstances, will give a correct difference of longitude of the two places, provided the eclipse be observed with telescopes of the same powers.

Taking the eclipses in the following tables, observed under these favourable circumstances as the standard, and comparing their results as given in the last column, with those of the others, it will be found, how much the latter are affected, by the eclipses having been observed when the atmosphere was hazy, or the planet very low, during twilight, or when the moon was near the planet, or the satellite not far from the body of Jupiter; and that even if correspondent observations had been taken, under favourable circumstances, at a known meridian, the difference of longitude given by the comparison would have been far from correct: the same eclipse observed at two places, under similar unfavourable circumstances, would possibly give a result near the truth; as the observations at both places would be affected in the same way, but probably not in an equal degree, as it is not likely there would be exactly the same degree of haze, the same strength of twilight, &c. &c. at both places; and therefore those observations taken under the same *favourable* circumstances can only be relied upon with certainty.

It may not be an easy matter to have telescopes at both places of precisely the same powers for these observations: at Madras we had two telescopes in use, constructed at the same time, in appearance precisely alike, and intended by DOLLOND to have been so in all respects; yet on repeated trials, one was found to have a decided advantage of several seconds over the other, shewing the emersions sooner, and the immersions later by that quantity. In order to do away the error arising from a difference in the powers of telescopes, *immersions* and *emersions* should be observed at both places; the difference of longitude will be as much greater than it ought to be by one series as less by the other, but the medium will be the correct difference of longitude of the places: it is possible also there may be some difference in the eyes of observers, any error which may arise from this source will also be done away by this method.

Hence it would appear, that in order to obtain a correct difference of longitude of two places from correspondent eclipses of the satellites of Jupiter, the circumstances at both places should be similar and favourable; and that the telescopes should have equal powers, or that both immersions and emersions should be observed, which indeed ought always to be done, where time will admit: also, that the circumstances being favourable at one place and not so at the other, a result very different from the truth will be obtained.

Eclipses of the Satellites of Jupiter.

Table I. *First Satellite.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
1794.		h ′ ″	h ′ ″	h ′ ″	h ′ ″	
Jan. 24	Im.	17 28 45	17 41 31	12 7 59	5 20 46	Planet clear and high, but near the ☽.
Feb. 9	Im.	15 41 24,5	15 56 4	10 20 55	5 20 29,5	Planet low and covered with a fog. Dark.
16	Im.	17 35 6	17 49 30	12 14 19	5 20 47	Clear. ☽ bright, though distant from ♃. Twilight.
Mar. 4	Im.	15 52 8,3	16 3 58,4	10 31 31	5 20 37.3	Clear, and the planet at a convenient altitude.
11	Im.	17 46 49	17 50 54	12 26 19	5 20 20	♃ clear and high, but near ☉ rise.
20	Im.	14 11 8,5	14 18 34,5	8 50 33	5 20 35,5	Planet rather low, and not very clear. The ☽ bright.
27	Im.	16 6 27	16 11 41	10 46 1	5 20 26	Hazy. Planet high.
Apr. 5	Im.	12 30 24,5	12 32 58	7 10 19	5 20 5,5	Planet low, but clear.
12	Im.	14 26 4	14 26 39	9 5 41	5 20 23	Clear. The ☽ up, but not very near ♃.
19	Im.	16 21 8	16 19 59	11 0 54	5 20 14	Planet high, but covered with haze and near the ☽.
28	Im.	12 44 41,6	12 41 53,5	7 24 27	5 20 14,6	Planet high. Dark, but the atmosphere hazy.
May 5	Im.	14 38 47	14 35 10	9 18 53	5 19 54	Planet high, but covered with a thin haze.
12	Im.	16 33 14,3	16 29 16	11 13 2	5 20 12,3	Clear. Planet high.
21	Im.	12 55 24,5	12 51 36,2	7 35 11	5 20 13,5	Clear. ♃ high. The ☽ up, but not bright or near ♃.
28	Im.	14 48 46	14 45 41	9 28 37	5 20 9	♃ high. A considerable undulation.
30	Im.	9 16 46,6	9 13 56	3 56 54	5 19 52,6	Planet rather low and misty.
June 4	Im.	16 41 41,3	16 39 40,7	11 21 44	5 19 57.3	Clear.
6	Im.	11 9 53	11 8 11	5 49 59	5 19 54	Clear. The ☽ up, but not near ♃.
July 1	Em.	7 58 0,5	8 1 25,2	2 37 22	5 20 38,5	Planet rather hazy; other circumstances favourable.
31	Em.	10 6 56,5	10 12 52,5	4 44 49	5 22 7.5	Planet high, but covered with haze.
Sep. 1	Em	6 51 15	6 50 54	1 30 30	5 20 45	Planet high, but hazy, and near the ☽.
24	Em.	7 14 10	7 5 58,6	1 53 21	5 20 49	Clear. Dark. Planet high.
Oct. 17	Em.	7 36 18	7 21 38	2 15 4	5 21 14	Planet high, but rather hazy.
Nov. 9	Em.	7 51 50	7 35 55,5	2 30 51	5 20 59	Planet low, but clear. Dark.
25	Em.	6 7 25,5	5 54 51,5	0 46 11	5 21 14,5	Planet rather hazy. Twilight.
1795.						
Feb. 28	Im.	17 7 55.4	17 20 40	11 47 32	5 20 23.4	Planet covered with haze.
Apr. 8	Im.	15 42 12	15 43 55	10 21 47	5 20 25	Rather hazy. ☽ up, and near the last quarter.
15	Im.	17 37 37	17 37 27	12 16 57	5 20 40	Clear. The planet high, but twilight.
24	Im.	14 1 13	13 59 9	8 40 28	5 20 45	Planet clear, though low. Dark.
May 1	Im.	15 55 38	15 52 28	10 34 54	5 20 44	Clear. ♃ high. The ☽ up, but not near the planet.
10	Im.	12 16 54,5	2 13 0,5	6 57 21	5 19 33,5	Clear.
17	Im.	14 11 37	14 7 41	8 50 52	5 20 45	Planet high, but hazy.
24	Im.	16 4 31	16 1 1	10 43 57	5 20 34	Planet high and clear.
July 4	Im.	8 51 23,5	8 55 19,6	3 31 11	5 20 12,5	Planet low, near the ☽, and tremulous.
9	Im.	16 16 17,5	16 21 04	10 56 2	5 20 15,5	Planet high, but rather hazy, and the ☽ up.
11	Im.	10 44 40	10 49 42	5 24 21	5 20 19	Planet high, and dark.
27	Em.	11 17 14.4	11 23 19,3	5 56 28	5 20 46,4	Clear, and the planet high. ☽ up, and about 45° from ♃.
Aug. 12	Em.	9 37 38,5	9 42 16,5	4 16 33	5 21 5.5	Planet high and hazy. Dark.
26	Em.	13 31 55	13 33 21	8 10 13	5 21 42	Planet near the ☽, and covered with a thin haze.
28	Em.	7 59 59.5	8 00 55	2 39 34	5 20 25.5	Clear, but ♃ near the ☽.
Sep. 4	Em.	9 57 44.5	9 56 27,7	4 37 10	5 20 34.5	Clear. Dark. Planet high.
11	Em.	11 55 37.5	11 51 57,7	6 35 11	5 20 26,5	Clear. Dark.
27	Em.	10 21 32	10 12 21,5	5 1 1	5 20 31	Clear, but the ☽ up, and near full.

Table I. *First Satellite continued.*

Day.	Im or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
		h ″	h ′ ″	h ′ ″	h ′ ″	
Oct. 6	Em.	6 49 10,5	6 37 14,5	1 28 27	5 20 43,5	Clear, and the planet high.
20	Em.	10 44 7,5	10 28 58	5 23 23	5 20 44,5	Clear. Dark, but the planet low.
Dec. 14	E.n.	7 30 33	7 25 43,4	2 9 55	5 20 38	Planet low, but clear. Dark.
1796.						
Apr. 3	Im.	17 12 6	17 15 2	11 51 12	5 20 54	Planet rather hazy, and near the ☽.
26	Im	17 26 56,3	17 29 31,2	12 6 11	5 20 45,3	Planet high and clear, but twilight.
May 12	Im.	15 43 29,3	15 39 32	10 22 59	5 20 30,3	Planet high, but hazy.
28	Im.	13 58 11,2	13 55 11	8 37 23	5 20 48,2	A thin haze over ♃. The ☽ near.
June 20	Im.	14 2 48	14 4 6,5	8 42 6	5 20 42	Planet high. A thin haze and moonlight.
July 6	Im.	12 15 38	12 20 4	6 54 52	5 20 46	Flying clouds. Dark. Planet high.
22	Im.	10 30 15	10 36 16,5	5 9 28	5 20 47	Planet low, but clear. A little tremulous. The ☽ near ♃.
29	Im	12 24 15	12 30 15	7 3 33	5 20 42	Clear. Planet high.
Sep. 13	Em.	15 14 3,6	15 9 23,6	9 53 8	5 20 55,6	Planet rather low, but clear.
22	Em.	11 40 44,5	11 32 58,5	6 20 8	5 20 36,5	Planet high. Rather hazy.
Oct. 1	Em.	8 8 3,5	7 57 23	2 47 8	5 20 55,5	Clear, and the planet high.
15	Em.	12 2 43	11 48 19	6 41 59	5 20 44	Planet high. Thin clouds.
17	Em.	6 31 58	6 17 13	1 11 17	5 20 41	Clear. The planet sufficiently high.
31	Em.	10 25 1,5	10 8 47,3	5 4 15	5 20 46,5	Clear. Planet high.
Nov. 9	Em.	6 50 17,4	6 34 26	1 28 44	5 21 33,4	Planet high, covered by a thin cloud,
23	Em.	10 38 36	10 25 57	5 17 45	5 20 51	Clear. The planet moderately high.
Dec. 2	Em.	7 0 31,2	6 50 40	1 39 34	5 20 57,2	Clear. Planet high. ☽ up, but far from ♃.
1798.						
Jan. 6	Em.	6 46 41,2	6 53 13,5	1 25 58	5 20 43,2	Clear. Planet high.
13	Em.	8 40 23,4	8 49 46,6	3 19 8	5 21 15,4	Planet high, but hid by a cloud at the precise time of the emersion, and probably 15″ afterwards.
29	Em	6 56 37,3	7 10 17,7	1 35 51	5 20 46,3	Clear. Planet sufficiently high. ☽ up, but far from ♃.
Feb. 5	Em	8 51 44,4	9 6 12,2	3 31 0	5 20 44,4	Planet rather low, but generally clear. Observation not satisfactory.
21	Em.	7 13 4,8	7 26 59,2	1 52 21	5 20 43,8	Planet high, but very tremulous and near the ☽.
Mar 16	Em.	7 36 34,5	7 45 16,3	2 13 36	5 22 58,5	Planet very low. Observation of little value in consequence.
Jun 12	Im.	17 31 21,5	17 30 53,5	12 11 23	5 19 58,5	Planet high. Rather hazy, about ☉ rise.
Aug. 6	Im.	14 9 36,4	14 15 2	8 48 21	5 21 15,4	Clear. Planet high, but about 15° above the ☽.
Oct. 7	Im.	13 1 48	12 49 27	7 40 24	5 21 24	Planet high. Clear.
16	Im.	9 26 12,3	9 11 44,5	4 4 53	5 21 19,3	Planet sufficiently high. Clear.
21	Im.	16 52 22,4	16 36 59,3	11 31 16	5 21 6,4	Clear. Planet high.
23	Im.	11 21 24,2	11 5 46,6	6 0 2	5 21 22,2	Clear. Planet high ☽ up, and near full.
30	Im.	13 15 58	12 59 45,6	7 54 41	5 21 17	Planet near the zenith. ☽ up, but far from ♃.
Nov. 15	Em	13 39 55,5	13 24 55,4	8 18 28	5 21 27,5	Planet high. Clear. Observation not satisfactory to 10 or 15″.
17	Em.	8 7 14	7 52 35	2 46 45	5 20 29	Clear. Planet high. ☽ up, but far from ♃.
24	Em.	10 0 3	9 47 13,5	4 39 32	5 20 31	Clear. Planet high. Moonlight.
Dec. 3	Em.	6 20 50	6 11 9,8	0 59 54	5 20 56	Planet high. A very thin cloud over the planet.
17	Em.	10 3 58,4	10 0 48,7	4 43 29	5 20 29,4	Clear. Planet near the zenith. Moonlight.
24	Em.	11 55 57,4	11 56 19	6 35 15	5 20 42,4	Planet high. Atmosphere somewhat hazy. Moonlight.
31	Em.	13 48 53,4	13 52 43	8 27 12	5 21 41,4	Planet low, and tremulous. Observation of little value.
1799.						
Jan. 9	Em.	10 7 57,2	10 15 43,5	4 47 37	5 20 20,2	Clear. Planet high.

Table I. *First Satellite continued.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
		h ′ ″	h ′ ″	h ′ ″	h ′ ″	
Jan. 16	Em.	12 1 20	12 11 44,5	6 40 36	5 20 44	Planet rather low and tremulous.
18	Em.	6 29 15,2	6 40 13,8	1 8 56	5 20 19,2	Planet high. ☽ about 30° from ♃.
25	Em	8 23 10,5	8 36 01,5	3 2 42	5 20 28,5	Clear. Planet high.
Feb. 1	Em.	10 17 45,5	10 31 49,5	4 57 10	5 20 35,5	Planet low, and covered with a thin haze.
10	Em.	6 41 54,7	6 56 32,8	1 21 19	5 20 35,7	Planet high. Clear. Moonlight.
17	Em.	8 38 1	8 52 21,6	3 17 27	5 20 34	Planet high. Clear. ☽ up, but far from ♃.
Aug. 2	Im.	15 42 16	15 48 5,7	10 22 31	5 19 45	♃ low and misty.
Sep. 10	Im.	14 18 5,2	14 14 44,6	8 57 25	5 20 40,2	Planet high. Clear. ☽ up, but opposite ♃.
Oct. 3	Im.	14 34 57,6	14 23 49,6	9 14 1	5 20 56,6	Planet high. Clear.
10	Im.	16 30 18,7	16 17 11,8	11 9 26	5 20 52,7	Planet high. Clear.
Nov. 20	Im.	9 24 14	9 10 12,6	4 2 58	5 21 16	Clear. Planet high.
Dec. 6	Im.	7 33 41,7	7 25 12,7	2 13 25	5 20 16,7	Planet rather low, but clear. Moonlight.
13	Im.	9 24 39,2	9 19 23,6	4 4 11	5 20 28,2	Clear. ♃ high. Sat. close to ♃. ☽ near full.
22	Em	7 56 13	7 55 21,8	2 33 42	5 22 31	Planet high, but hazy.
27	Em.	15 18 12,3	15 20 00,6	9 56 46	5 21 26,3	Planet high, rather tremulous.
29	Em.	9 45 42	9 48 22	4 24 29	5 21 13	Clear. Planet high.
31	Second Satellite.					
	Em.	7 12 27,6	7 16 03	1 49 40	5 22 47,6	Planet high and clear. ☽ up. Observation not satisfactory.
1800.						
Jan. 5	Em.	11 36 55,2	11 42 54,4	6 15 42	5 21 13,2	Clear. Planet high. Moonlight.
14	Em	7 56 17,4	8 5 51	2 35 28	5 20 49,4	Clear. Planet high.
21	Em	9 48 44,5	10 0 33,6	4 28 3	5 20 41,5	Clear. Planet high.
30	Em	6 10 45	6 24 30	0 49 49	5 20 56	Planet high, rather hazy. Twilight.
Feb. 6	Em.	8 4 41,5	8 19 12	2 44 8	5 20 33,5	Planet near the zenith. ☽ up, uncertain to 6″.
13	Em	10 0 5	10 14 40	4 39 12	5 20 53	Planet high, rather hazy.
Mar. 8	Em	10 18 16,2	10 29 17	4 57 22	5 20 54,2	Planet high. ☽ up.
24	Em.	8 42 25	8 48 49,2	3 21 38	5 20 47	Clear. Planet high.
Apr. 9	Em.	7 7 25	7 9 00,6	1 46 8	5 21 17	Planet high. ☽ full, but opposite ♃.
16	Em.	9 4 25	9 4 8,2	3 43 26	5 20 59	Planet rather low: tremulous. Dark.
May 2	Em	7 27 13	7 24 0	2 6 0	5 21 13	Planet rather low, somewhat hazy.
25	Em.	7 45 0,4	7 41 32,5	2 20 53	5 24 7,4	Planet very low. A thick haze.
30	Im.	14 28 9	14 17 15,4	9 7 37	5 20 32	Air clear. ♃ high. The object glass rather dimmed by the dew.
1801.						
Jan. 15	Im.	14 27 29,3	14 37 25,1	9 4 58	5 22 31,3	Clear. Planet high.
17	Im.	8 52 59,2	9 3 31,2	3 32 51	5 20 8,2	Clear. Planet high.
24	Em.	13 1 22	13 13 56,3	7 39 57	5 21 25	Planet near the zenith. Position very aukward.
26	Em	7 28 47	7 41 45,7	2 8 4	5 20 43	Clear. Planet high. Moonlight.
Feb. 2	Em.	9 21 17	9 35 17	4 1 4	5 20 13	Planet high. ☽ just rising.
9	Em	11 15 8,6	11 29 45,3	5 54 52	5 20 16,6	Planet near the zenith. Clear.
18	Em.	7 38 22	7 52 41	2 18 8	5 20 14	Planet high. ☽ up, but far from ♃.
23	Em.	15 6 34,3	15 20 15,3	9 44 38	5 21 56,3	Planet very low and tremulous.
25	Em.	9 33 28,6	9 46 53,1	4 13 34	5 19 54,6	Moonlight. Planet near the zenith.
Mar. 4	Em	11 29 51,3	11 41 52	6 9 39	5 20 12,3	Clear. Planet high. ☽ up, but far from ♃.
6	Em.	5 59 38,5	6 11 14,5	0 38 42	5 20 56,5	Planet high. Somewhat hazy. Twilight.
11	Em.	13 26 18	13 36 33	8 6 6	5 20 12	Planet rather low and tremulous.
13	Em.	7 55 37,4	8 5 23,2	2 35 14	5 20 23,4	Clear. Planet high.

Table I. *First Satellite continued.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
		h ′ ″	h ′ ″	h ′ ″	h ′ ″	
Mar. 27	Em.	11 49 49,6	11 55 20,2	6 29 6	5 20 43,6	Planet rather low, but clear.
29	Em.	6 18 52,7	6 23 50,3	0 58 21	5 20 31,7	Clear. Planet high. Twilight.
Apr. 5	Em.	8 15 48	8 18 35,5	2 55 23	5 20 25	Planet high. Rather hazy.
12	Em.	10 15 37	10 16 24,8	4 52 19	5 23 18	Clear. ♃ high. The observation not satisfactory.
28	Em.	8 35 4,4	8 32 25,3	3 14 24	5 20 40,4	Planet high, but hazy.
May 21	Em.	8 49 53,4	8 46 6	3 29 8	5 20 45,4	Clear, and planet high. The ☽ up.
Nov. 3	Im.	15 32 11	15 15 56,5	10 13 43	5 18 28	Planet high. A thin haze.
19	Im.	13 47 38	13 33 19,3	8 27 15	5 20 23	Planet low and tremulous.
26	Im.	15 34 46	15 22 25	10 18 49	5 15 57	Planet high, but covered with a thick haze.
Dec. 3	Im.	17 30 6	17 20 20,3	12 9 36	5 20 30	Clear. ♃ in the zenith. Twilight just appearing.
26	Im.	17 27 59	17 29 5,7	12 7 24	5 20 35	Clear. ♃ high, but near the ☽. Twilight beginning.
1802.						
Jan. 18	Im.	17 27 0	17 37 53,8	12 6 16	5 20 44	Planet high. Rather hazy. ☽ up. Twilight.
Feb. 5	Im.	10 7 25	10 21 50	4 46 55	5 20 30	Clear. Planet high.
19	Im.	13 54 59,2	14 9 11,7	8 34 38	5 20 21,2	Clear. Planet high. ☽ near ♃.
28	Em	12 34 10,3	12 47 10,5	7 13 30	5 20 40,3	Clear. Planet high.
Mar. 7	Em.	14 29 11	14 40 31,6	9 9 9	5 20 2	Planet high. Clear.
9	Em.	8 58 17	9 9 11	3 38 6	5 20 11	Clear. Planet high.
16	Em.	10 54 22,7	11 3 20,4	5 34 14	5 20 8,7	Clear. Planet high. ☽ near ♃, and almost full.
Apr. 8	Em.	11 14 37,5	11 16 36,7	5 52 55	5 21 42,5	Planet high. Hazy.
17	Em.	7 38 34,4	7 38 11	2 18 24	5 20 10,4	Clear. ♃ in the zenith. ☽ rising.
June 25	Em.	8 15 23	8 17 23,4	2 53 9	5 22 4	Planet high, but misty. Telescope not steady.
Oct 30	Im.	16 34 29	16 17 19,5	11 13 57	5 20 32	Planet rather low and tremulous. Air clear.
Nov 22	Im.	16 41 31	16 27 55	11 20 55	5 20 36	Planet rather low, but clear.
Dec. 15	Im.	16 45 48,6	16 41 18,6	11 20 55	5 24 33,6	Clear. ♃ near the ☽. Uncertain observation.
31	Im.	14 48 44,6	14 52 9,6	9 28 22	5 20 22,6	Clear. Planet high.

Table II. *Second Satellite.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
		h ′ ″	h ′ ″	h ′ ″	h ′ ″	
1794.						
Feb. 27	Im.	15 17 10	15 30 5	9 58 37	5 18 33	Planet low, and covered with haze.
Mar. 31	Im.	15 4 40,3	15 8 42	9 45 14	5 19 26,3	Clear. Planet high.
Apr. 7	Im.	17 41 2,5	17 42 57,7	12 22 3	5 18 59,5	Clear. Planet high. Near ⊙ rise.
25	Im.	12 10 53	12 8 34	6 52 17	5 18 36	Clouds covered the planet, after the satellite had faded, probably a very few seconds before it immerged.
May 2	Im.	14 47 27	14 44 13,5	9 27 32	5 19 55	Planet near the meridian, but very tremulous.
9	Im.	17 22 6	17 18 13,5	12 2 11	5 19 55	Clear. Planet high. Strong twilight.
27	Im.	11 46 43,3	11 43 30,3	6 26 38	5 20 5,3	Clear. Planet high. Dark.
June 3	Im.	14 20 13	14 18 1	8 59 41	5 20 32	Clear. Planet high. Dark.
10	Im.	16 52 37	16 51 43	11 32 26	5 20 11	Clear, but ♃ rather low. Beginning of the twilight.
21	Em.	11 21 7	11 22 29	6 0 51	5 20 16	Clear. Planet high. Satellite emerged close to the planet.
July 16	Em.	8 17 13,6	8 22 49,5	2 57 8	5 20 5,6	Planet high. Very little hazy.
Aug. 17	Em.	7 56 1,6	7 59 39,5	2 35 54	5 20 7,6	Planet high. Clear. Dark.
24	Em.	10 33 19,4	10 35 15	5 12 43	5 20 36,4	Planet high. Very thin haze. Dark.
Oct. 20	Em.	7 36 1	7 20 49,5	2 14 43	5 21 18	Clear. Dark.
1795.						
Apr. 1	Im.	16 34 57,5	16 38 44	11 16 1	5 18 56,5	Clear. Dark. Planet sufficiently high.
May 3	Im.	16 26 50	16 23 26	11 6 28	5 20 22	Hazy. Planet high. ☽ up, but far from ♃.
28	Im.	13 32 6	13 29 1	8 11 48	5 20 18	Planet high. Rather hazy. The ☽ up, but far from ♃.
Aug. 11	Em.	7 24 42	7 29 27,5	2 4 26	5 20 16	Clear. Dark. Wind high.
18	Em.	10 1 18,4	10 4 47	4 41 8	5 20 10,4	Clear. ♃ high. Dark.
Sep. 12	Em.	7 12 28	7 8 31,5	1 52 13	5 20 15	Clear. Planet high. Dark.
Oct. 14	Em.	7 2 32	6 48 33,5	1 41 15	5 21 17	Clear. ♃ high.
Nov. 15	Em.	6 44 31	6 29 24	1 23 13	5 21 18	Clear. The ☽ up.
1796.						
Apr. 26	Im.	15 41 47,5	15 39 13,5	10 20 47	5 21 00,5	Planet low and tremulous. The ☽ up.
May 28	Im.	15 29 9	15 26 9	10 8 21	5 20 48	Hazy. Planet high. The ☽ near ♃.
July 24	Im.	12 15 26	12 21 30	6 54 58	5 20 28	Planet high. Somewhat hazy. The ☽ near ♃.
Aug. 25	Im.	12 5 16	12 6 46	6 44 28	5 20 48	Planet high. Clear. Satellite close to the planet.
Sept. 12	Em.	9 31 34	9 27 20	4 10 55	5 20 39	Planet high. A thin haze.
19	Em.	12 10 3	12 3 19	6 49 40	5 20 23	Clear. Planet high.
26	Em.	14 48 48	14 39 38,5	9 28 20	5 20 28	Planet low and tremulous.
Oct. 14	Em.	9 24 50,5	9 10 40,7	4 3 34	5 21 16,5	Planet covered at the time by a very thin cloud.
21	Em.	12 1 22	11 45 55	6 40 56	5 20 26	Planet sufficiently high.
Nov. 15	Em.	9 7 33	8 52 36	3 46 19	5 21 14	Planet high. A thin haze. ☽ up, but not near ♃.
1798.						
Jan. 12	Em	7 59 23,3	8 8 23,3	2 38 21	5 21 2,3	Flying clouds. Planet high.
Mar. 17	Em	7 22 34	7 30 58,5	2 0 5	5 22 29	Planet very low and tremulous.
June 24	Im.	17 21 19	17 23 25,3	11 58 24	5 22 55	Planet high. Somewhat hazy. Twilight.
Sep. 28	Im.	16 51 32	16 41 51,6	11 30 2	5 21 30	Planet high. Hazy. The ☽ near ♃.
Oct. 9	Im.	8 51 1,5	8 38 11,2	3 29 35	5 21 26,5	Clear. ♃ low, and rather tremulous.
23	Im.	14 9 2	13 53 23,5	8 47 39	5 21 23	Planet near the zenith. Moonlight.
Nov. 17	Em.	13 45 18	13 30 42	8 22 26	5 22 52	Clear. Planet high.
Dec. 12	Em.	10 47 12,5	10 41 37,7	5 25 34	5 21 38,5	Planet high. Clear.

Table II. *Second Satellite continued.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
1799.		h ' "	h ' "	h ' "	h ' "	
Jan. 6	Em.	7 45 58,2	7 52 25,6	2 25 8	5 20 50,2	Planet high. Clear.
13	Em.	10 20 13	10 29 33	4 59 34	5 20 39	Planet high. ☽ near ♃.
Feb. 7	Em.	7 25 0,2	7 39 33,9	2 3 49	5 21 11,2	Planet high, rather hazy.
14	Em.	10 1 25,6	10 15 58	4 40 28	5 20 57,6	Planet low and tremulous. ☽ up.
Mar. 11	Em.	7 12 30	7 22 41,3	1 51 40	5 20 50	Clear. Planet high. Moonlight.
July 20	Im.	16 42 6,7	16 48 2,8	11 20 43	5 21 23,7	Planet high. Hazy. Observation uncertain.
Sep. 15	Im.	13 35 42,6	13 30 37,6	8 14 34	5 21 8,6	Planet rather low. Hazy. ☽ up, and near the full.
Oct. 17	Im.	13 28 32	13 13 52,3	8 6 55	5 21 37	♃ high. Clouds after the satellite had faded, possibly 15 or 20" before the time. ☽ up.
24	Im.	16 6 7,2	15 50 22	10 44 29	5 21 38,2	Planet near the zenith. ☽ up.
Nov. 18	Im.	13 13 3,4	12 58 35,8	7 51 54	5 21 9,4	Planet near the zenith. ☽ up. Observation uncertain to 10".
Dec. 6	Im.	7 39 25,6	7 30 56,8	2 18 47	5 20 38,6	Planet rather low, but clear. Moonlight.
13	Im.	10 10 49	10 5 34,4	4 53 1	5 17 48	♃ high, and the ☽ near. Clouds covered the planet after the satellite had faded, possibly 30 or 40" before the time. Observation of no value in consequence.
1800.						
Jan. 7	Em.	9 46 30	9 53 19,6	4 24 31	5 21 59	Planet high. Clear. ☽ near ♃.
Feb. 8	Em.	9 28 35	9 43 10,8	4 7 49	5 20 46	Planet high. ☽ near ♃.
Mar. 12	Em.	9 22 44,5	9 32 42	4 1 54	5 20 50,5	Planet high. ☽ up, but far from ♃. Rather hazy.
Nov. 19	Im.	14 45 30,5	14 31 15	9 24 1	5 21 29,5	Clear. Planet high.
26	Im.	17 18 36,7	17 6 22	11 57 16	5 21 20,7	Planet near the zenith. Twilight.
Dec. 14	Im.	11 40 37	11 35 45,7	6 19 1	5 21 36	Clear. Planet sufficiently high.
1801.						
Feb. 2	Em.	8 26 3,4	8 40 10,6	3 3 53	5 22 10,4	Clear. Planet. Observation not satisfactory.
9	Em.	11 2 30	11 17 6,7	5 40 56	5 21 34	Clear. Planet near the zenith. Observation not satisfactory.
Mar. 6	Em.	8 17 9,3	8 28 44	2 56 17	5 20 52,3	Clear. ♃ near the zenith.
13	Em.	10 56 45,6	11 6 29,3	5 36 1	5 20 44,6	Planet near the zenith. Clear.
20	Em.	13 37 10,2	13 44 49,3	8 15 58	5 21 12,2	Planet very low, and tremulous.
Oct. 19	Im.	16 46 35	16 31 38	11 26 19	5 20 16	Clear. Planet high. Twilight.
Dec. 15	Im.	13 3 19,2	12 58 53,1	7 42 10	5 21 9,2	Clear. Planet high.
29	Im.	18 4 18,6	18 6 54,6	12 43 1	5 21 17,6	Clear. Planet high. Twilight.
1802.						
Jan. 9	Im.	9 50 27,5	9 57 56,3	4 29 37	5 20 50,5	♃ rather low and tremulous.
23	Im.	14 55 7,7	15 7 24,3	9 34 30	5 20 37,7	♃ in the zenith. Clear. Moonlight. The object glass dimmed by dew.
Feb. 10	Im.	9 21 39,5	9 36 18	4 0 48	5 20 51,5	Clear. Planet high. Moonlight.
17	Im.	11 56 50	12 11 13,8	6 36 58	5 19 52	Planet near the zenith. ☽ full, and very close to ♃.
Mar. 7	Em.	9 22 13,3	9 33 37	4 1 17	5 20 56,3	Planet high. Clear. Observation good.
May 17	Em.	11 50 57,3	11 46 59,4	6 29 10	5 21 47,3	♃ low. Hazy. ☽ up.

Table III. *Third Satellite.*

Day.	Im. or Em.	Time of Observation.		Time by the Ephemeris.	Longitude of Madras by the Tables.	Circumstances of Weather, &c.
		Apparent.	Mean.			
		h ′ ″	h ′ ″	h ′ ″	h ′ ″	
1794.						
Feb. 15	Im.	15 57 20,5	16 11 49	10 36 42	5 20 38.5	Planet clear and high. ☽ near full, but far from ♃.
Mar. 30	Im.	15 53 1,5	15 57 21	10 31 52	5 21 9,5	Planet dimmed in a small degree by vapour.
May 5	Im.	11 50 39	11 47 3	6 29 4	5 21 35	Hazy. Planet rather low. Dark.
	Em.	14 36 56	14 33 19,5	9 16 38	5 20 18	Planet high. A thin haze. Dark.
12	Im.	15 49 10	15 45 12	10 27 30	5 21 40	Clear. Planet high.
July 23	Im.	7 25 59	7 32 0	2 6 32	5 19 27	Clear. Planet high. The satellite close to ♃.
Sep. 4	Im.	7 33 32	7 32 13	2 13 15	5 20 17	Rather hazy. Planet high. The ☽ near.
1795.						
May 27	Em.	14 38 43	14 35 31	9 17 4	5 21 39	Clear. Planet high.
July 16	Im.	14 58 24	15 4 01,5	9 34 45	5 23 39	Clear. Planet high. Dark.
Aug. 14	Em.	10 31 33	10 35 49,5	5 10 24	5 21 9	Clear. Planet high. Dark.
Sep. 26	Im.	7 19 8	7 10 20	1 57 2	5 22 6	Planet high. Air not very clear. The ☽ up.
Nov. 1	Em.	7 8 9	6 51 54	1 45 50	5 22 19	Clear. Planet high.
8	Im.	7 36 28	7 20 26	2 13 13	5 23 15	Clear. Dark.
Dec. 14	Em.	7 5 44	7 0 54	1 43 6	5 22 38	Clear. Dark.
1796.						
May 12	Im.	15 41 59,5	15 38 2	10 20 16	5 21 43,5	Hazy. Planet high.
Oct. 10	Em.	7 24 19	7 11 06,7	1 59 28	5 24 51	Hazy. Planet high. The ☽ up, but not near ♃.
Nov. 22	Em.	7 35 8	7 21 49	2 9 44	5 25 24	Planet high. Rather hazy.
1798.						
Jan. 26	Em.	7 55 14,3	8 8 22	2 31 29	5 23 45,3	Clear. Planet high. The ☽ up, but far from ♃.
Sep. 5	Em.	12 39 6	12 37 22,6	7 15 54	5 23 12	Clear. Planet high.
12	Em.	16 42 40	16 38 30,5	11 18 35	5 24 5	Clear. ♃ near the zenith.
Oct. 18	Im.	11 6 49,5	10 51 58	5 38 47	5 28 2,5	Clear. ♃ high. Moonlight.
	Em.	12 56 22	12 41 29,5	7 30 52	5 25 30	Clear. ♃ near the zenith. Observation uncertain to 15 or 20.″
25	Em.	16 59 44	16 43 51,3	11 32 30	5 27 14	Clear. ♃ high and near the ☽. The satellite close to ♃.
1799.						
Jan. 5	Im.	6 52 56	6 58 5,3	1 26 0	5 26 5,6	Clear. Planet high.
	Em.	8 48 27	8 54 29,5	3 25 25	5 23 2	Clear. Planet high.
12	Im.	10 50 21,4	10 59 19,3	5 24 18	5 26 3,4	Clear. Planet high.
Apr. 1	Im.	7 9 23	7 13 15	1 44 1	5 25 22	Clear. Planet high.
Oct. 4	Im.	15 17 57	15 6 30,5	9 53 43	5 24 14	Planet high. Clear.
1800.						
Jan. 27	Em.	9 34 4	9 47 18,5	4 12 43	5 21 21	☽ near the zenith. Clear.
1801.						
Jan. 13	Im.	10 3 45,7	10 12 54	4 44 14	5 19 31,7	Planet high. Clear.
1802.						
Feb. 4	Im.	8 54 56	9 9 15,4	3 38 7	5 16 49	Clear. Planet high.
Nov. 3	Im.	16 42 7	16 25 52,8	11 26 39	5 15 28	Air clear, but ♃ rather low.

XXIII. *Electro-Chemical Researches, on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia.*

By Humphry Davy, *Esq. Sec. R. S. M. R. I. A.*

Read June 30th, 1808.

1. *Introduction.*

In the Philosophical Transactions for 1807, Part I. and 1808, Part I. I have detailed the general methods of decomposition by electricity, and stated various new facts obtained in consequence of the application of them.

The results of the experiments on potash and soda, as I stated in my last communication to the Society, afforded me the strongest hopes of being able to effect the decomposition both of the alkaline and common earths; and the phenomena obtained in the first imperfect trials made upon those bodies countenanced the ideas that had obtained from the earliest periods of chemistry, of their being metallic in their nature.[*]

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[*] BECCHER is the first chemist, as far as my reading informs me, who distinctly pointed out the relations of metals to earthy substances, see Phys. subt. Lipsiæ, 4to. p. 61. He was followed by STAHL, who has given the doctrine a more perfect form. BECCHER's idea was that of an universal elementary earth, which, by uniting to an inflammable earth, produced all the metals, and under other modifications formed stones. STAHL admitted distinct earths which he supposed might be converted into metals by combining with phlogiston; see STAHL Fundament. Chym. p. 9. 4to. and Conspect. Chem. 1. 77. 4to.—NEUMAN gives an account of an elaborate series of unsuccessful experiments which he made to obtain a metal from quicklime. LEWI's

Many difficulties however occurred in the way of obtaining complete evidence on this subject: and the pursuit of the enquiry has required much labour and a considerable devotion of time, and has demanded more refined and complicated processes than those which had succeeded with the fixed alkalies.

The earths like the fixed alkalies are non-conductors of electricity; but the fixed alkalies become conducting by fusion: the infusible nature of the earths, however, rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxygene, made it unavailing to act upon them in solution in water; and the only methods that proved successful, were those of operating upon them by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity, in metallic alloys, so as to obtain evidences of their nature and properties.

NEUMAN's Chem. Works, 2d. edit. vol. i. p. 15. The earlier English chemical philosophers seem to have adopted the opinion of the possibility of the production of metals from common earthy substances; see BOYLE, vol. i. 4to. p. 564, and GREW, Anatomy of Plants, lec. ii. p. 242. But these notions were founded upon a kind of alchemical hypothesis of a general power in nature of transmuting one species of matter into another. Towards the end of the last century the doctrine was advanced in a more philosophical form; BERGMAN suspected barytes to be a metallic calx, Præf. Sciagrap. Reg. Min. & Opusc. iv. 212. BARON supported the idea of the probability of alumine being a metallic substance, see Annales de Chemie, vol. x. p. 257.—LAVOISIER extended these notions, by supposing the other earths metallic oxides. Elements, 2d edit. KERR's translation, p. 217. The general enquiry was closed by the assertion of TONDI and RUPRECHT, that the earths might be reduced by charcoal; and the accurate researches of KLAPROTH and SAVARESI, who proved by the most decisive experiments, that the metals taken for the bases of the earths were phosphurets of iron, obtained from the bone ashes and other materials employed in the experiment, Annales de Chemie, vol. viii p. 18. and vol. x. p. 257, 275. Amidst all these hypotheses, potash and soda were never considered as metallic in their nature; LAVOISIER supposed them to contain azote; nor at that time were there any analogies to lead that acute philosopher to a happier conjecture.

I delayed for some time laying an account of many of the principal results which I obtained before the Society, in the hopes of being able to render them more distinct and satisfactory; but finding that for this end a more powerful battery, and more perfect apparatus than I have a prospect of seeing very soon constructed, will be required, I have ventured to bring forwards the investigation in its present imperfect state; and I shall prefer the imputation of having published unfinished labours, to that of having concealed any new facts from the scientific world, which may tend to assist the progress of chemical knowledge.

2. *Methods employed for decomposing the alkaline Earths.*

Barytes, strontites, and lime, slightly moistened, were electrified by iron wires under naphtha, by the same methods, and with the same powers as those employed for the decomposition [*] of the fixed alkalies. In these cases, gas was copiously evolved, which was inflammable; and the earths where in contact with the negative metallic wires became dark coloured, and exhibited small points having a metallic lustre, which, when exposed to air, gradually became white; they became white likewise when plunged under water, and when examined in this experiment by a magnifier, a greenish powder seemed to separate from them, and small globules of gas were disengaged.

In these cases there was great reason to believe that the earths had been decomposed; and that their bases had combined with the iron, so as to form alloys decomposible by the oxygene of air or water; but the indistinctness of the effect, and the complicated circumstances required for it, were such as to compel me to form other plans of operation.

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[*] See page 4.

The strong attraction of potassium for oxygene, induced me to try whether this body might not detach the oxygene from the earths, in the same manner as charcoal decomposes the common metallic oxides.

I heated potassium in contact with dry pure lime, barytes, strontites, and magnesia, in tubes of plate glass; but as I was obliged to use very small quantities, and as I could not raise the heat to ignition without fusing the glass, I obtained in this way no good results. The potassium appeared to act upon the earths and on the glass, and dark brown substances were obtained, which evolved gas from water; but no distinct metallic globules could be procured: from these circumstances, and other like circumstances, it seemed probable, that though potassium may partially de-oxygenate the earths, yet its affinity for oxygene, at least at the temperature which I employed, is not sufficient to effect their decomposition.

I made mixtures of dry potash in excess and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit in the same manner as that I employed for obtaining the metals of the alkalies. My hopes were, that the potassium, and the metals of the earths might be de-oxygenated at the same time, and enter into combination in alloy.

In this way of operating, the results were more distinct than in the last: metallic substances appeared, less fusible than potassium, which burnt the instant after they had formed, and which by burning produced a mixture of potash and the earth employed; I endeavoured to form them under naphtha, but without much success. To produce the result at all, required a charge by the action of nitric acid, which the state of the batteries did not permit me often to employ;* and the metal was generated only in very minute films, which could not be detached by fusion, and which were instantly destroyed by exposure to air.

I had found in my researches upon potassium, that when a mixture of potash and the oxides of mercury, tin, or lead, was electrified in the Voltaic circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium was obtained; the attraction between the common metals and the potassium apparently accelerating the separation of the oxygene.

The idea that a similar kind of action might assist the decomposition of the alkaline earths, induced me to electrify mixtures of these bodies and the oxide of tin, of iron, of lead, of silver, and of mercury; and these operations were far more satisfactory than any of the others.

A mixture of two-thirds of barytes and one-third of oxide of silver very slightly moistened was electrified by iron wires; an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point. When the iron wire to which this substance adhered was plunged into water containing a little alum in solution, gas was disengaged, which proved to be hydrogene; and white clouds which were found to be sulphate of barytes, descended from the point of the wire.

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* The power of this combination, though it consisted of one hundred plates of copper and zinc of six inches, and one hundred and fifty of four inches, at this time was not more than equal to that of a newly constructed apparatus of one hundred and fifty, of four inches. It had been made for the demonstrations in the Theatre of the Royal Institution in 1803; and since that time had been constantly employed in the annual courses of Lectures, and had served in different parts, for the numerous experiments on the decomposition of bodies by electricity, detailed in the Bakerian Lectures for 1806 and 1807, and a number of the plates were destroyed by corrosion. I mention these circumstances, because many chemists have been deterred from pursuing experiments on the decomposition of the alkalies and the earths, under the idea that a very powerful combination was required for the effect. This, however, is far from being the case; all the experiments detailed in the text may be repeated by means of a Voltaic battery, containing from one hundred to one hundred and fifty plates of four or six inches.

A mixture of barytes and red oxide of mercury, in the same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance, that produced barytes by exposure to air, with the absorption of oxygene; and which occasioned the evolution of hydrogene from water, leaving pure mercury, and producing a solution of barytes.

Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air or water, with like phenomena; but the quantities of metallic substances obtained were exceedingly minute; they appeared as mere superficial formations surrounding the point of the wire, nor did they increase after the first few minutes of electrization, even when the process was carried on for some hours.

These experiments were made previous to April, 1808, at which time the batteries were so much injured by constant use, as no longer to form an efficient combination. The enquiry was suspended for a short time: but in May I was enabled to resume it, by employing a new and much more powerful combination, constructed in the Laboratory of the Royal Institution, and consisting of five hundred pairs of double plates of six inches square.

When I attempted to obtain amalgams with this apparatus, the transmitting wires being of platina, of about $\frac{1}{40}$ of an inch in diameter; the heat generated was so great as to burn both the mercury and basis of the amalgam at the moment of its formation; and when by extending the surfaces of the conductors, this power of ignition was modified, yet still the amalgam was only produced in thin films, and I could not obtain globules sufficiently large to submit to distillation. When the transmitting wires were of iron of the same thickness, the iron acquired the temperature of ignition, and combined with the bases of the earths in preference to the mercury, and metallic alloys of a dark grey colour were obtained, which acted on water with the evolution of hydrogene, and were converted into oxide of iron, and alkaline earths.

Whilst I was engaged in these experiments, in the beginning of June, I received a letter from Professor BERZELIUS of Stockholm, in which he informed me that in conjunction with Dr. PONTIN, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them, and that in this way he had obtained amalgams of the metals of these earths.

I immediately repeated these operations with perfect success; a globule of mercury, electrified by the power of the battery of 500, weakly charged, was made to act upon a surface of slightly moistened barytes, fixed upon a plate of platina. The mercury gradually became less fluid, and after a few minutes was found covered with a white film of barytes;

and when the amalgam was thrown into water, hydrogene was disengaged, the mercury remained free, and a solution of barytes was formed

The result with lime, as these gentlemen had stated, was precisely analogous.

That the same happy methods must succeed with strontites and magnesia, it was not easy to doubt, and I quickly tried the experiment.

From strontites I obtained a very rapid result; but from magnesia, in the first trials, no amalgam could be procured. By continuing the process however, for a longer time, and keeping the earth continually moist, at last a combination of the basis with mercury was obtained, which slowly produced magnesia by absorption of oxygene from air, or by the action of water.

All these amalgams I found might be preserved for a considerable period under naphtha. In a length of time, however, they became covered with a white crust under this fluid. When exposed to air, a very few minutes only were required for the oxygenation of the bases of the earths. In water the amalgam of barytes was most rapidly decomposed: that of strontites and that of lime next in order: but the amalgam from magnesia, as might be expected from the weak affinity of the earth for water, very slowly changed; when a little sulphuric acid was added to the water, however, the evolution of hydrogene, and the production and solution of magnesia were exceedingly rapid, and the mercury soon remained free.

I was inclined to believe that one reason why magnesia was less easy to metallize than the other alkaline earths, was its insolubility in water, which would prevent it from being presented in the nascent state, detached from its solution at the negative surface. On this idea I tried the experiment, using moistened sulphate of magnesia, instead of the pure earth; and I found that the amalgam was much sooner obtained. Here the magnesia was attracted from the sulphuric acid, and probably deoxygenated and combined with the quicksilver at the same instant.

The amalgams of the other bases of the alkaline earths, could, I found, be obtained in the same manner from their saline compounds.

I tried in this way very successfully, muriate and sulphate of lime, the muriate of strontites, and of barytes, and nitrate of barytes. The earths separated at the deoxygenating surface, there seemed instantly to undergo decomposition, and seized upon by the mercury, were in some measure defended from the action of air, and from the contact of water, and preserved by their strong attraction for this metal.

III. *Attempts to procure the Metals of the alkaline Earths; and on their Properties.*

To procure quantities of amalgams sufficient for distillation, I combined the methods I had before employed, with those of M. M. Berzelius and Pontin.

The earths were slightly moistened, and mixed with one-third of red oxide of mercury, the mixture was placed on a plate of platina, a cavity was made in the upper part of it to receive a globule of mercury, of from fifty to 60 grains in weight, the whole was covered by a film of naphtha, and the plate was made positive, and the mercury negative, by a proper communication with the battery of five hundred.

The amalgams obtained in this way, were distilled in tubes of plate glass, or in some cases in tubes of common glass. These tubes were bent in the middle, and the extremities were enlarged, and rendered globular by blowing, so as to serve the purposes of a retort and receiver.

The tube after the amalgam had been introduced, was filled with naphtha, which was afterwards expelled by boiling, through a small orifice in the end corresponding to the receiver, which was hermetically sealed when the tube contained nothing but the vapour of naphtha, and the amalgam.

I found immediately that the mercury rose pure by distillation from the amalgam, and it was very easy to separate a part of it; but to obtain a complete decomposition was very difficult.

For this nearly a red heat was required, and at a red heat the bases of the earths instantly acted upon the glass, and became oxygenated. When the tube was large in proportion to the quantity of amalgam, the vapour of the naphtha furnished oxygene sufficient to destroy part of the bases: and when a small tube was employed, it was difficult to heat the part used as a retort sufficient to drive off the whole of the mercury from the basis, without raising too highly the temperature of the part serving for the receiver, so as to burst the tube.*

In consequence of these difficulties, in a multitude of trials, I obtained only a very few successful results, and in no case could I be absolutely certain that there was not a minute portion of mercury still in combination with the metals of the earths.

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* When the quantity of the amalgam was about fifty or sixty grains, I found that the tube could not be conveniently less than one-sixth of an inch in diameter, and of the capacity of about half a cubic inch.

In the best result that I obtained from the distillation of the amalgam of barytes, the residuum appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness, in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis, in the first degree of oxygenation.*

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* From this fact, compared with other facts that have been stated, p. 336, it may be conjectured, that the basis of barytes has a higher affinity for oxygene than sodium; and hence, probably the bases of the earths will be more powerful instruments for detecting oxygene, than the bases of the alkalies.

I have tried a number of experiments on the action of potassium on bodies supposed simple, and on the undecompounded acids. From the affinity of the metal for oxygene, and of the acid for the substance formed, I had entertained the greatest hopes of success. It would be inconsistent with the object of this paper to enter into a full detail of the methods of operation; I hope to be able to state them fully to the Society at a future time, when they shall be elucidated by further researches; I shall now merely mention the general results, to shew that I have not been tardy in employing the means which were in my power, towards effecting these important objects.

When potassium was heated in muriatic acid gas, as dry as it could be obtained by common chemical means, there was a violent chemical action with ignition; and when the potassium was in sufficient quantity, the muriatic acid gas wholly disappeared, and from one-third to one-fourth of its volume of hydrogene was evolved, and muriate of potash was formed.

On fluoric acid gas, which had been in contact with glass, the potassium produced a similar effect; but the quantity of hydrogene generated was only one-sixth or one-seventh of the volume of gas, and a white mass was formed, which principally consisted of fluate of potash and silex, but which emitted fumes of fluoric acid when exposed to air.

When boracic acid, prepared in the usual manner, that had been ignited, was heated in a gold tube with potassium, a very minute quantity of gas only was liberated, which was hydrogene, mixed with nitrogene, (the last probably from the common air in the tube); borate of potash was formed, and a black substance, which became white by exposure to air.

When exposed to air, it rapidly tarnished, and fell into a white powder, which was barytes When this process was conducted in a small portion of air, the oxygene was found absorbed, and the nitrogene unaltered; when a portion of it was introduced into water, it acted upon it with great violence and sunk to the bottom, producing in it barytes; and hydrogene was generated. The quantities in which I obtained it were too minute for me to be able to examine correctly, either its physical or chemical properties. It sunk rapidly in water, and even in sulphuric acid, though surrounded by globules of hydrogene, equal to two or three times its volume; from which it seems probable, that it cannot be less than four or five times as heavy as water. It flattened by pressure, but required a considerable force for this effect.

In all these instances there is great reason to believe that the hydrogene was produced from the water adhering to the acids; and the different proportions of it in the different cases, are a strong proof of this opinion. Admitting this idea, it seems that muriatic acid gas must contain at least one-eighth or one-tenth of its weight of water; and that the water oxygenates in the experiment a quantity of potassium, sufficient to absorb the whole of the acid.

In the cases of fluoric and boracic acids, there is probably a decomposition of these bodies; the black substance produced from the boracic acid is similar to that which I had obtained from it by electricity. The quantities that I have operated upon, have been as yet too small to enable me to separate and examine the products, and till this is done, no ultimate conclusion can be drawn.

The action of potassium upon muriatic acid gas, indicates a much larger quantity of water in this substance, than the action of electricity in Dr. Henry's elaborate experiments; but in the one instance the acid enters into a solid salt, and in the other it remains aeriform; and the difficulty of decomposition by electricity, must increase in proportion as the quantity of water diminishes, so that at the apparent maximum of electrical effect, there is no reason to suppose the gas free from water.

Those persons who have supposed hydrogene to be the basis of muriatic acid may, perhaps, give another solution of the phenomena, and consider the experiment I have detailed as a proof of this opinion.

The metal from strontites sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxydation.

The metal from lime, I have never been able to examine exposed to air or under naphtha. In the case in which I was able to distil the quicksilver from it to the greatest extent, the tube unfortunately broke, whilst warm, and at the moment that the air entered, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quicklime.

The metal from magnesia seemed to act upon the glass, even before the whole of the quicksilver was distilled from it. In an experiment in which I stopped the process before the mercury was entirely driven off, it appeared as a solid, having the same whiteness and lustre as the other metals of the earths. It sunk rapidly in water, though surrounded by globules of gas, producing magnesia, and quickly changed in air, becoming covered with a white crust, and falling into a fine powder, which proved to be magnesia.

In several cases in which amalgams of the metals of the earths, containing only a small quantity of mercury were obtained, I exposed them to air on a delicate balance, and always found that during the conversion of metal into earth, there was a considerable increase of weight.

I endeavoured to ascertain the proportions of oxygene, and bases, in barytes and strontites, by heating amalgams of them in tubes filled with oxygene, but without success. I satisfied myself, however, that when the metals of the earths were burned in a small quantity of air they absorbed oxygene, gained weight in the process, and were in the highly caustic or unslacked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids.

The evidence for the composition of the alkaline earths is then of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar, the inflammable matters in all cases separating at the negative surface in the Voltaic circuit, and the oxygene at the positive surface.

These new substances will demand names; and on the same principles as I have named the bases of the fixed alkalies, potassium and sodium, I shall venture to denominate the metals from the alkaline earths barium, strontium, calcium, and magnium; the last of these words is undoubtedly objectionable, but magnesium * has been already applied to metallic manganese, and would consequently have been an equivocal term.

IV. *Enquiries relative to the Decomposition of Alumine, Silex, Zircone, and Glucine.*

I tried the methods of electrization and combination with quicksilver, and the common metals, by which I had succeeded in decomposing the alkaline earths, on alumine and silex; but without gaining distinct evidences of their having undergone any change in the processes.

Obliged to seek for other means of acting upon them, it was necessary to consider minutely their relations to other bodies, and to search for analogies by which the principles of research might be guided.

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* Bergman. Opusc. tom. ii. p. 200.

Alumine very slowly finds its point of rest at the negative pole, in the electrical circuit; but silex, even when diffused in its gelatinous state through water, rests indifferently at the negative or positive poles.

From this indifference to positive and negative electrical attractions, following the general order of facts, it might be inferred, that if these bodies be compounds, the electrical energies of their elements are nearly in equilibrium; and that their state is either analogous to that of insoluble neutral salts, or of oxides nearly saturated with oxygene.

The combinations of silex and alumine, with acids and alkalies, as well as their electrical powers, were not inconsistent with either of these ideas; for in some respects they resemble in physical characters, fluate and phosphate of lime, as much as in others, they approach to the oxides of zinc and tin.

On the idea that silex might be an insoluble neutrosaline compound, containing an unknown acid or earth, or both, and capable of being resolved into its secondary elements, in the same manner as sulphate of barytes, or fluate of lime, I made the following experiments.

Two gold cones,* connected by moistened amianthus, were filled with pure water, and placed in the electrical circuit, a small quantity of carefully prepared and well washed silex was introduced into the positive cone: the action was kept up from a battery of two hundred plates, for some hours till nearly half of the fluid in each cone was exhausted; the remainders were examined; the fluid in the cone containing the silex was strongly acid; that in the opposite cone was strongly alkaline; the two fluids were passed through bibulous paper, and mixed together, when a precipitate fell down, which proved to be silex.

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* The same as those described in Phil. Trans. 1807, p. 6.

On the first view of the subject, it appeared probable that this silex had been formed by the union of the acid and the alkaline matter in the two cones, and that the experiment demonstrated a decomposition and recomposition of silex; but before such a conclusion could be made, many points were to be determined.

It was possible that the acid might be nitric acid, produced as in other electrical experiments of a similar nature, and that this acid might have dissolved silex, which was precipitated by the alkaline matter at the other pole, which might be either potash used for dissolving the silex, which had adhered to it, notwithstanding the processes of lixiviation in acids, or ammonia produced in consequence of the presence of the atmosphere; or if potash was present, it was likewise possible that the silex might have been carried over in solution, with this alkali, from the positive to the negative surface.

Minute experiments were instituted and completed in the same manner as those detailed in the Philosophical Transactions for 1807, p. 7. which soon proved that there was no reason to suppose that the silex had been changed in these experiments.

The acid proved to be nitric acid, which under the electrical action seemed to have dissolved the silex; the alkali turned out to be principally fixed alkali; and that it was merely an accidental ingredient, and not a constituent of the silex, appeared from this circumstance, that when the same portion of silex was long electrified, by degrees it lost its power of affording the substance in question.[*]

This result having taken place, the same plan of operation was not pursued with respect to alumine, which resembles a saline compound less than silex, and the method which I now adopted of acting upon these bodies, was on the supposition of their being inflammable substances so highly saturated with oxygene as to possess little or no positive electricity.

Alumine and silex have both a strong affinity for potash and soda; now supposing them to be oxides, it was reasonable to conclude that the oxygene, both in the alkalies and the earths, must be passive as to this power, which must consequently be referred to their bases, and on this notion it was possible that it might be made to assist their decomposition by electricity.

After this reasoning, I fused a mixture of one part of silex, and six of potash in a platina crucible, and preserved the mixture fluid, and in ignition, over a fire of charcoal; the crucible was rendered positive from the battery of five hundred, and a rod of platina, rendered negative, was brought in contact with the alkaline menstruum. At the moment of contact there was a most intense light; when the rod was plunged into the liquid an effervescence took place, and globules which burnt with a brilliant flame rose to the surface, and swam upon it in a state of combustion. In a few minutes, when the mixture was cool, the platina bar was removed: after as much as possible of the alkali and silex had been detached from it by a knife, there remained brilliant metallic scales round it, which instantly became covered with a white crust in the air, and some of which inflamed spontaneously. The platina appeared much corroded, and of a darker tint than belongs to the pure metal. When it was plunged into water it strongly effervesced: the fluid that came from it was alkaline; when a few drops of muriatic acid were added to the solution, a white cloudiness occurred, which various trials demonstrated, depended upon the presence of silex.

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[*] If silex that has been carefully washed, after precipitation by muriatic acid from liquor silicum, be moistened, and acted on by mercury negatively electrified, the mercury soon contains a notable quantity of potassium. Well washed alumine that has been precipitated from alum by carbonate of soda, affords by the same treatment sodium and potassium, so that the powers of electrochemical analysis are continually demonstrating the imperfection of the common chemical methods of separating bodies from each other. The purest boracic acid which can be obtained from borax by chemical decomposition, by electrical analysis is shewn to contain both soda, and the decomposing acid employed in the process; and hence the experiment on the action of the boracic acid and potassium, page 343, may possibly be explained without assuming its decomposition.

A similar mixture of potash and alumine was experimented upon in the same manner, and the results were perfectly analogous; there adhered to the rod of platina a film of a metallic substance, which rapidly decomposed water, and afforded a solution which deposited alumine by the action of an acid.

I tried several forms of this experiment, with the hopes of being able to obtain a sufficient quantity of the metallic matter from the platina, so as to examine it in a separate state; but I was not successful. It was always in superficial scales, which oxidated, becoming white and alkaline, before it could be detached in the air; it instantly burnt when heated, and could not be fused under naphtha or oil.

I tried similar experiments with mixtures of soda and alumine, and soda and zircone, and used iron as the negatively electrified metal. In all these cases, during the whole process of electrization, abundance of globules, which swam in a state of inflammation on the fused mass, were produced. And in the mixture, when cooled, small laminæ of metal were found of the colour of lead, and less fusible than sodium, which adhered to the iron; they acted violently upon water, and produced soda and a white powder, but in quantities too small to be minutely examined.

I endeavoured to procure an alloy of potassium, and the bases of the earths, from mixtures of potash, silex, and alumine, fused by electricity, and acted on by the positive and negative surfaces in the same manner as pure potash, in experiments for the decomposition of that substance; but I obtained no good results. When the earths were in quantities equal to one-fourth or one-fifth of the alkali, they rendered it so highly non-conducting, that it was not easy to affect it by electricity, and when they were in very minute portions, the substance produced had the characters of pure potassium.

I heated small globules of potassium, in contact with silex and alumine, in tubes of plate glass filled with the vapour of naphtha: the potassium seemed to act at the same time upon the glass and the earths, and a grayish opaque mass, not possessed of metallic splendour was obtained, which effervesced in water, depositing white clouds. Here it was possible that the potash had been converted wholly or partly into protoxide, by its action upon the earths; but as no globule was obtained, and as the plate glass alone might have produced the effect, no decided inference of the decomposition of the earths can be drawn from the process.

I shall now mention the last trials that I made with respect to this object.

Potassium, amalgamated with about one-third of mercury, was electrified negatively under naphtha, in contact with silex very slightly moistened, by the power of five hundred; after an hour the result was examined. The potassium was made to decompose water, and the alkali formed neutralized by acetous acid; a white matter, having all the appearance of silex precipitated, but in quantity too small for accurate examination.

I tried the same method of action upon alumine and glucine, and obtained a cloudiness, more distinct than in the case of silex, by the action of an acid upon the solution obtained from the amalgam.

Zircone exposed in the same manner to the action of electricity, and the attraction of potassium, furnished still more satisfactory results, for a white and fine powder, soluble in sulphuric acid, and which was precipitated from sulphuric acid by ammonia, separated from the amalgam that had been obtained, by the action of water.

From the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude that alumine, zircone, glucine, and silex are, like the alkaline earths, metallic oxides, for on no other supposition is it easy to explain the phenomena that have been detailed.

The evidences of decomposition and composition, are not, however of the same strict nature as those that belong to the fixed alkalies and alkaline earths; for it is possible, that in the experiments in which the silex, alumine, and zircone appeared to separate during the oxidation of potassium and sodium, their bases might not actually have been in combination with them, but the earths themselves, in union with the metals of the alkalies, or in mere mechanical mixture. And out of an immense number of experiments which I made of the kind last detailed, a very few only gave distinct indications of the production of any earthy matter; and in cases when earthy matter did appear, the quantity was such as rendered it impossible to decide on the species.

Had I been so fortunate as to have obtained more certain evidences on this subject, and to have procured the metallic substances I was in search of, I should have proposed for them the names of silicium, alumium, zirconium, and glucium.

V. *On the production of an Amalgam from Ammonia, and on its Nature and Properties.*

IN the communication from Professor BERZELIUS and Dr. PONTIN, which I have already referred to, a most curious and important experiment on the deoxydation and amalgamation of the compound basis of ammonia is mentioned, which these ingenious gentlemen regard as a strict proof of the idea I had formed of its being an oxide with a binary basis.

Mercury, negatively electrified in the VOLTAIC circuit, is placed in contact with solution of ammonia. Under this agency it gradually increases in volume, and when expanded to four or five times its former dimensions, becomes a soft solid.

And that this substance is composed of the deoxygenated compound basis of ammonia and mercury, they think is proved; 1. By the reproduction of quicksilver and ammonia, with the absorption of oxygene, when it is exposed to air; and secondly, by its forming ammonia in water, whilst hydrogene is evolved, and the quicksilver gradually becomes free.

An operation, in which hydrogene and nitrogene exhibit metallic properties, or in which a metallic substance is apparently composed from its elements, cannot fail to fix the attention of chemists: and the peculiar interest which it offered in its relations to the general theory of electrochemical science, induced me to examine the circumstances connected with it minutely and extensively.

In repeating the process of the Swedish chemists, I found that to form an amalgam from fifty or sixty grains of mercury, in contact with saturated solution of ammonia, required a considerable time, and that this amalgam greatly changed even in the short period required for removing it from the solution.

I was however able, in this mode of operating, to witness all the results they have stated, and I soon found simple and more easy means of producing the effect, and circumstances under which it could be more distinctly analysed.

The experiments which I have detailed in the BAKERIAN lecture for 1806, proved that ammonia is disengaged from the ammoniacal salts, at the negative surface in the VOLTAIC circuit; and I concluded that under this agency, it may be acted on in what is called the nascent state, when it was reasonable to conclude it would be more readily deoxygenated and combined with quicksilver.

On this view of the subject, I made a cavity in a piece of muriate of ammonia; into this a globule of mercury, weighing about fifty grains, was introduced. The muriate was slightly moistened, so as to be rendered a conductor, and placed on a plate of platina, which was made positive in the circuit of the large battery. The quicksilver was made negative by means of a platina wire. The action of the quicksilver on the salt was immediate; a strong effervescence with much heat took place. The globule in a few minutes had enlarged to five times its former dimensions, and had the appearance of an amalgam of zinc; and metallic crystallizations shot from it, as a centre, round the body of the salt. They had an arborescent appearance, often became coloured at their points of contact with the muriate; and when the connection was broken, rapidly disappeared, emitting ammoniacal fumes, and reproducing quicksilver.

When a piece of moistened carbonate of ammonia was used, the appearances were the same, and the amalgam was formed with equal rapidity. In this process of deoxydation, when the battery was in powerful action, a black matter formed in the cavity, which there is every reason to believe was carbonaceous matter from the decomposition of the carbonic acid of the carbonate.*

The strong attraction of potassium, sodium, and the metals of the alkaline earths for oxygene, induced me to examine whether their deoxydating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and the result was very satisfactory.

When mercury, united to a small quantity of potassium, sodium, barium, or calcium, was made to act upon moistened muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

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* The black matter which separates at the negative surface in the electrical experiments on the decomposition of potash or soda, and which some experimenters have found it difficult to account for, is I find carbonaceous, and dependent upon the presence of carbonic acid in the alkali.

As in these cases, however, a portion of the metal used for the deoxydation always remained in union in the compound; in describing the properties of the amalgam from ammonia, I shall speak only of that procured by electrical means.

The amalgam from ammonia, when formed at the temperature of 70° or 80, is a soft solid, of the consistence of butter; at the freezing temperature it becomes firmer, and a crystallized mass, in which small facets appear, but having no perfectly defined form.* Its specific gravity is below 3, water being one.

When exposed to air it soon becomes covered with a white crust, which proves to be carbonate of ammonia.

When thrown into water it produces a quantity of hydrogene, equal to about half its bulk, and in consequence of this action the water becomes a weak solution of ammonia.

When it is confined in a given portion of air, the air enlarges considerably in volume, and the pure quicksilver re-appears. Ammoniacal gas, equal to one and a half or one and three-fifths of the volume of the amalgam is found to be produced, and a quantity of oxygene equal to one-seventh, or one-eighth of the ammonia disappears.†

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* From the facet I suspect the form to be cubical. The amalgam of potassium crystallizes in cubes as beautiful, and in some cases as large, as those of bismuth.

† This experiment confirms the opinions I have stated concerning the quantity of oxygene in ammonia; but as water is present, as will be immediately shewn, the data for proportions are not perfectly correct.

When thrown into muriatic acid gas, it instantly becomes coated with muriate of ammonia, and a small quantity of hydrogene is disengaged.

In sulphuric acid it becomes coated with sulphate of ammonia and sulphur.

I attempted by a variety of modes to preserve this amalgam. I had hoped by submitting it to distillation out of the contact of air, or water, or bodies which could furnish oxygene, to be able to obtain the deoxygenated substance which had been united to the quicksilver in a pure form; but all the circumstances of the experiment opposed themselves to such a result.

It is well known to persons accustomed to barometrical experiments, that mercury after being once moistened, retains water with great perseverance, and can only be freed from it by boiling; and in the cases of the decomposition of ammonia, when a soft amalgam had been kept continually moist, both internally and externally for some time, it could not be expected that all the water adhering to it should be easily removed.

I wiped the amalgam as carefully as possible with bibulous paper; but even in this process a considerable portion of the ammonia was regenerated; I attempted to free it from moisture by passing it through fine linen, but a complete decomposition was effected, and nothing was obtained but pure quicksilver.

The whole quantity of the basis of ammonia combined in sixty grains of quicksilver, as is evident from the statements that have been made, does not exceed $\frac{1}{200}$ part of a grain, and to supply oxygene to this scarcely $\frac{1}{1000}$ part of a grain of water would be required, which is a quantity hardly appreciable, and which merely breathing upon the amalgam would be almost sufficient to communicate.

Hence, when an amalgam, which had been wiped by means of bibulous paper, was introduced into naphtha, it decomposed almost as rapidly as in the air, producing ammonia and hydrogene.

In oils it evolved hydrogene, and generated ammoniacal soap; and when it was introduced into a glass tube, closed by a cork, gas was rapidly formed, and the mercury remained free; and this gas, when examined, was found to consist of from about two-thirds to three-fourths ammonia, and the remainder hydrogene.*

That more moisture sometimes existed attached to the amalgam, when wiped as dry as possible by bibulous paper, than was sufficient for the effect of decomposition, I soon found by an experiment of distillation.

About a quarter of a cubic inch of an amalgam nearly solid was wiped very dry, and introduced into a small tube: in this tube it was heated till the gaseous matter had expelled the quicksilver; the tube was then closed, and suffered to cool, when moisture, which proved to be a saturated solution of ammonia, had precipitated upon it.

I have mentioned that the amalgams obtained from ammonia, by means of the metals of the fixed alkalies or alkaline earths, seemed to contain much more ammoniacal basis in combination than those procured by electricity: and when they are combined with the metals of the fixed alkalies or of the earths in any considerable quantities, they are much more permament.

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* In the experiment of the action of the amalgam upon air, the oxygene is probably absorbed by nascent hydrogene, and reproduces water, which is dissolved by the ammonia.

Triple compounds of this kind, when carefully wiped, scarcely produce any ammonia under naphtha, or oil, and may be preserved for a considerable time in closed glass tubes, a little hydrogene being the only product evolved from them.

I heated a triple amalgam obtained from ammonia by potassium, and which had been wiped by bibulous paper in a dry plate-glass tube over mercury; a considerable elevation of temperature was required before any gaseous matter was emitted, but the heat was raised till gas was rapidly formed, and the whole of the amalgam expelled from the tube: in cooling, the mercury rose very quickly in it, so that a great part of the gaseous matter had been either mercury or water, in vapour, or something which the mercury had absorbed in cooling. The small quantity which was permanent, did not equal one half the volume of the amalgam.

On the idea that this gas might be a compound of hydrogene and nitrogene in the state of deoxygenation, I mixed a small quantity of oxygene gas with it, but no change of volume took place; I then exposed it to naphtha, when one half of it was absorbed, which by the effect the naphtha produced upon turmeric must have been ammonia; the remaining gas analyzed was found to consist of the oxygene that had been introduced, and of hydrogene and nitrogene to each other in the proportion of nearly four to one.

At first I was perplexed by this result, which seemed to prove the production of ammonia, independent of the presence of any substance which could furnish oxygene to it, and to shew that its amalgamation was merely owing to its being freed from water, and combined with hydrogene: but a satisfactory solution of the difficulty soon offered itself. Exposing the triple amalgam procured from ammonia by potassium to a concentrated solution of ammonia, I found that it had very little action upon it, and introducing the amalgam moistened by it into a glass tube, it had nearly the same permanency as the amalgam which had been wiped before it was introduced, a little hydrogene only being evolved; but on heating the tube gaseous matter was rapidly generated, which proved to consist of two-thirds ammonia, and one-third hydrogene.

In the instance in which the amalgam had been wiped, a small quantity of solution of ammonia, and perhaps of potash, must have adhered to it; and though the amalgam does not act upon this powerfully at common temperatures, yet when the water is raised in vapour, it tends to oxygenate both the basis of ammonia, and potassium, and hence hydrogene is evolved, and volatile alkali produced.

I distilled an amalgam procured by potassium from ammonia, in a tube filled with the vapour of naphtha, and hermetically sealed, in the same manner as in the experiments for obtaining the metals of the earths, but in this case I procured ammonia, hydrogene, and nitrogene only, and pure mercury; and the residuum was potassium, which acted powerfully on the glass tube.

In another experiment of the same kind, I kept one part of the tube cool by ice, at the time the other part was strongly heated, but nothing condensible except mercury was produced, and the elastic products were the same as in the former instance.

I endeavoured to procure an amalgam from ammonia, to which no moisture could be supposed to adhere, by heating an amalgam of potassium in ammoniacal gas. The amalgam became covered with a film of potash, but it did not enlarge in its dimensions, and a considerable quantity of non-absorbable gas, which was found to consist of five parts of hydrogene, and one of nitrogene, was produced. The amalgam after this operation did not emit ammonia by exposure to air, hence it seems probable, that for the deoxygenation of ammonia, and the combination of its basis with mercury, the alkali must be in the nascent state, or at least in that condensed form in which it exists in ammoniacal salts, or solutions.

VI. *Some Considerations of general Theory, connected with the Metallization of the Alkalies and the Earths.*

THE more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about $\frac{1}{12000}$ part of its weight of new matter, is rendered a solid, yet has its specific gravity diminished from 13.5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

It is scarcely possible to conceive that a substance which forms with mercury so perfect an amalgam, should not be metallic in its own nature;* and on this idea to assist the discus-

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* The nature of the compounds of sulphur and phosphorus with mercury, favours this opinion; these inflammable bodies by combination, impair its metallic properties; cinnabar is a non conductor, and it would seem from PELLETIER's experiments, Ann. de Chemie, vol. xiii. p. 125, that the phosphuret of mercury is not metallic in its characters; charcoal is a conductor, and in plumbago carbon approaches very

discussion concerning it, it may be conveniently termed ammonium.

But on what do the metallic properties of ammonium depend?

Are hydrogene and nitrogene both metals in the aëriform state, at the usual temperatures of the atmosphere, bodies of the same character, as zinc and quicksilver would be in the heat of ignition?

Or are these gases, in their common form, oxides, which become metallized by deoxydation?

Or are they simple bodies not metallic in their own nature, but capable of composing a metal in their deoxygenated, and an alkali in their oxygenated state?

These problems, the second of which was stated by Mr. Cavendish to me, and the last of which belongs to Mr. Berzelius, offer most important objects of investigation.

I have made some experiments in relation to them, but as yet unsuccessfully. I have heated the amalgam of potassium, in contact with both hydrogene and nitrogene, but without attaining their metallization; but this fact cannot be considered as decisively for or against any one of these conjectures.

I mentioned in the Bakerian Lecture for 1807, that a modification of a phlogistic chemical theory might be defended on the idea that the metals and inflammable solids, usually called simple, were compounds of the same matter as that existing in

near to a metal in its characters, so that the metallic nature of steel does not militate against the reasoning in the text. The only facts which I am acquainted with, that do militate against it, are the metallic characters of some of the sulphurets and phosphurets of the imperfect metals.

hydrogene, with peculiar unknown bases, and that the oxides, alkalies, and acids were compounds of the same bases with water, and that the phænomena presented by the metals of the fixed alkalies might be explained on this hypothesis.

The same mode of reasoning may be applied to the facts of the metallization of the earths and ammonia, and perhaps with rather stronger evidences in its favour, but still it will be less, distinct and simple, than the usually received theory of oxygenation, which I have applied to them.

The general facts of the combustion, and of the action of these new combustible substances upon water, are certainly most easily explained on the hypothesis of LAVOISIER; and the only good arguments in favour of a common principle of inflammability, flow from some of the novel analogies in electrochemical science.

Assuming the existence of hydrogene in the amalgam of ammonium, its presence in one metallic compound evidently leads to the suspicion of its combination in others. And in the electrical powers of the different species of matter, there are circumstances which extend the idea to combustible substances in general. Oxygene is the only body which can be supposed to be elementary, attracted by the positive surface in the electrical circuit, and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxygene. Hydrogene is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxygene; may not then the different inflammable bodies, supposed to be simple, contain this as a common element?

Should future experiments prove the truth of this hypothesis, still the alkalies, the earths, and the metallic oxides will belong to the same class of bodies. From platina to potassium there is a regular order of gradation as to their physical and chemical properties, and this would probably extend to ammonium, could it be obtained in the fixed form. Platina and gold in specific gravity, degree of oxidability, and other qualities, differ more from arsenic, iron, and tin, than these last do from barium and strontium. The phænomena of combustion of all the oxidable metals are precisely analogous. In the same manner as arsenic forms an acid by burning in air, potassium forms an alkali and calcium an earth; in a manner similar to that in which osmium forms a volatile and acrid substance by the absorption of oxygene, does the amalgam of ammonium produce the volatile alkali; and if we suppose that ammonia is metallized, by being combined with hydrogene, and freed from water, the same reasoning will likewise apply to the other metals, with this difference, that the adherence of their phlogiston or hydrogene, would be exactly in the inverse ratio of their attraction for oxygene. In platina * it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any

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* The common metallic oxides are lighter than their bases, but potash and soda are heavier; this fact may be explained on either theory; the density of a compound will be proportional to the attraction of its parts. Platina, having a weak affinity for oxygene, cannot be supposed to condense it in the same degree as potassium does; or if platina and potassium be both compounds of hydrogene, the hydrogene must be attracted in platina, with an energy infinitely greater than in potassium. Sulphuric acid is lighter than sulphur; but phosphoric acid (where there is a stronger affinity) is heavier than phosphorus. The oxide of tin (wood tin) is very little inferior to tin in specific gravity. In this instance the metallic base is comparatively light, and the attraction for oxygene strong; and in a case when the metal is much lighter and the attraction for oxygene stronger, it might be expected a priori that the oxide would be heavier than the base.

of the metals without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalies, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

Whatever new lights new discoveries may throw upon this subject, still the facts that have been advanced, shew that a step nearer at least has been attained towards the true knowledge of the nature of the alkalies and the earths.*

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* Since the facts in this Paper were communicated to the Royal Society, I have seen an account of some very curious experiments of M.M. Gay Lussac, and Thenard, (in Number 148 of the Moniteur, for 1808, which I have just received,) from one of which they have concluded, "that potassium may be a compound of hydrogene and potash."

These gentlemen are said to have heated potassium in ammonia, and found that the ammonia was absorbed, and that hydrogene gas equal to two-thirds of its volume appeared, and that the potassium by this process had become of a grayish-green colour. By heating this grayish-green substance considerably, two-fifths of the ammonia were again emitted, with a quantity of hydrogene and nitrogene corresponding to one-fifth more, and by adding water to the mixture, and heating it very strongly again, they obtained the remainder of the ammonia, and nothing but potash was left.

In these complex processes, the phænomena may be as easily explained on the idea of potassium being a simple, as that of its being a compound substance; nor when the facts that have been stated in this paper and those about to be stated, are considered, can the view of these distinguished chemists, as detailed in the notice referred to, be at all admitted.

Potash, as I have found by numerous experiments, has no affinity for ammonia, for it does not absorb it when heated in it; it is not therefore (allowing their theory) possible to conceive that a substance having no attraction for potash, should repel from it a substance which is intimately combined with it, and which can be separated in no other way.

A part of the hydrogene evolved in their experiment, may be furnished by water contained in the ammonia; but it is scarcely possible that the whole of it can be derived from this source, for on such an idea the ammonia must contain more than half its weight of water. There is however no evidence that the whole of the hydrogene may

Something has been separated from them which adds to their weight; and whether it be considered as oxygene, or as

not be furnished by the decomposition of the volatile alkali itself. Potassium in its first degree of oxygenation, may have an affinity for nitrogene, or potassium may expel a portion of hydrogene at the moment of its combination with ammonium; and as the whole of the ammonia cannot be regenerated without the presence of water; hydrogene and a little oxygene may be furnished to the remaining elements of the ammonia, from the water, and oxygene to the potassium.

Even before the conclusion was formed, that a metallic substance is decomposed in this experiment, it should have been proved that the nitrogene had not been altered.

That mere potash, combined with hydrogene, cannot form potassium, is I think shewn by an experiment which I tried, in consequence of the important fact lately ascertained by M. M. Gay Lussac, and Thenard, of the deoxydation of potash by iron.

An ounce of potash was kept in ignition for some time in an iron tube, ground into a gun barrel in which one ounce and a half of iron turnings were ignited to whiteness; a communication was opened, by withdrawing a wire which closed the tube containing the potash, between that alkali and the metal.

As the potash came in contact with the iron, gaseous matter was developed, which was received in a proper apparatus, and though some of it was lost by passing through the potash into the atmosphere, yet nearly half a cubic foot was preserved, which proved to be hydrogene. In the tube were found two products, one in the quantity of a few grains, containing potassium, combined with a small quantity of iron, and which had sublimed in the operation, and the other a fixed white metallic substance which consisted of an alloy of iron and potassium.

The first of these substances burnt when thrown upon water; and in its other characters resembled pure potassium, except that its specific gravity was greater, its colour less brilliant, and when it tarnished in the atmosphere, it became of a much deeper colour than pure potassium.

Now potash that has been ignited, is the purest form known of this alkali; but on M. M. Gay Lussac's and Thenard's theory, this potash must contain water, not only sufficient to furnish hydrogene to metallize the alkali, but likewise the quantity disengaged: dry potash then, as it is procured in our experiments, must on this theory be a compound, containing a considerable quantity of matter which can furnish hydrogene; and what would be its form or properties if deprived of this matter we are wholly unable to judge, which brings this question to the general question discussed in the text.

water, the inflammable body is less compounded, than the uninflammable substance resulting from its combustion.

Potassium I find may be produced readily from dry ignited potash in electrical experiments; and the result of the combustion of potassium in oxygene gas is an alkali, so dry that it produces violent heat, and ebullition when water is added to it.

In M.M. Gay Lussac's and Thenard's experiment on the action of potassium on ammonia, the hydrogene disengaged in the first process, and that existing in the ammonia disengaged in the second process, exactly equals the whole quantity contained in the ammonia. But there is no proof of any hydrogene being disengaged from the potassium, for the ammonia lost is not generated, nor potash formed, but by the addition of a substance, consisting of oxygene and hydrogene; and as the three bodies concerned in this experiment are potassium, ammonia, and water, the result ought to be potash, ammonia, and a quantity of hydrogene, equal to that evolved by the mere action of water on potassium, which is said to be the case.

Even if there were no other proofs, the chemical properties of potassium are so wholly unlike those that might be expected from a compound of potash and hydrogene, that they are almost sufficient to decide the question. Potassium acts upon water with much more energy than potash, and produces much more heat in it; and yet if a compound of hydrogene, the affinity of potash for water must be diminished by its affinity for hydrogene, to say nothing of the quantity of heat, which ought (on the common theory of capacity for heat) to be carried off by this light inflammable gas.

Potassium burns in carbonic acid, and precipitates charcoal from it; whereas hydrogene electrized with carbonic acid, converts it into gaseous oxide of carbon.

Potash has a very slight attraction for phosphorus; but potassium has a very strong affinity for it, so as to separate it from hydrogene, and according to M.M. Gay Lussac and Thenard, with the phænomena of inflammation. Potash has no affinity for arsenic, yet from the experiments of these gentlemen, it appears that potassium separates arsenic from arseniated hydrogene; and hydrogene, which is supposed by them to exist in both compounds, can have no affinity for hydrogene, nor can hydrogene in one form, be supposed capable of separating arsenic from hydrogene in another form.

Could not the experiment of M. M. Gay Lussac and Thenard be explained, except on the supposition of the hydrogene being derived from the potassium, it would be a distinct fact in favour of the revival of the theory of phlogiston. It would not prove, however, that potassium is composed of hydrogene and potash, but that it is composed of hydrogene and an unknown basis; and that potash is this basis united to water.

Other hypotheses might be formed upon the new electrochemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained. Certain electrical states always coincide with certain chemical states of bodies. Thus acids are uniformly negative, alkalies positive, and inflammable substances highly positive; and as I have found, acid matters when positively electrified, and alkaline matters when negatively electrified, seem to loose all their peculiar properties and powers of combination. In these instances the chemical qualities are shewn to depend upon the electrical powers; and it is not impossible that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms.[*]

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[*] Phil. Trans. 1807, Part I. p. 23. The amalgam obtained from ammonia offers difficulties to both the phlogistic and antiphlogistic hypotheses. If we assume the phlogistic hypothesis, then we must assume that nitrogene, by combining with one fourth of its weight of hydrogene can form an alkali, and by combining with one-twelfth more, can become metallic. If we reason on the antiphlogistic hypothesis, we must assert, that though nitrogene has a weaker affinity for oxygene than hydrogene, yet a compound of hydrogene and nitrogene is capable of decomposing water.

The first assumption is however by far the most contradictory to the order of common chemical facts; the last, though it cannot be wholly removed, is yet lessened by analogies. Thus alloys in general, and inflammable compounds, are more oxidable than the simple substances that compose them. Sulphuret of iron at common temperatures decomposes water with facility, whereas sulphur under the same circumstances, has no action on water, and iron a very small one. The compound of phosphorus and hydrogene, is more inflammable than either of its constituents.

Should a new theory of the dependence of the chemical forms of matter upon electrical powers be established, the facts belonging to ammonium would admit of a more easy solution. Ammonium might be supposed to be a simple body, which by combining with different quantities of water, and in different states of electricity, formed nitrogene, ammonia, atmospherical air, nitrous oxide, nitrous gas, and nitric acid.

Water, on this idea, must be supposed a constituent part of all the different gasses; but its electricities in oxygene and hydrogene would probably be the very reverse of what they have been supposed by M. Ritter, and some ingenious English enquirers.

I venture to hint at these notions: but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtile powers of matter are but just beginning to be considered; and all general views concerning them, must as yet rest upon feeble and imperfect foundations.

Whatever be the fate of the speculative part of the enquiry, the facts however will, I hope, admit of many applications, and explain some phænomena in nature.

The metals of the earths cannot exist at the surface of the globe; but it is very possible that they may form a part of the interior; and such an assumption would offer a theory for the phænomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat,* and would probably lead to a general hypothesis in geology.

The luminous appearance of those meteors connected with the fall of stones, is one of the extraordinary circumstances of these wonderful phænomena. This effect may be accounted

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Water positively electrified would be hydrogene, water negatively electrified, oxygene; and as in the physical experiments of temperature, ice, added to certain quantities of steam by an equilibrium of heat produces water, so in the chemical experiment of the generation of water the positive and negative electricity of oxygene and hydrogene in certain proportions would annihilate each other, and water alone be the result. At all events ammonium, whether simple or compound, must be considered as owing its attraction for oxygene to its highly positive electrical state, which is shewn by its powerful determination to the negative surface in the Voltaic circuit.

[*] Let it be assumed that the metals of the earths and alkalies, in alloy with common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water, must produce the effect of subterranean fire, and a product of earthy and stony matter analogous to lavas.

for, by supposing that the substances which fall, come into our atmosphere in a metallic state; and that the earths they principally consist of are a result of combustion; but this idea has not the slightest connexion with their origin or causes.

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